~~CONFIDENTIAL~~

~~RESTRICTED~~

INDEXED

74 K

M I N U T E S A N D P R O C E E D I N G S

of the twenty-first meeting of the

U. S. ARMED FORCES-NRC VISION COMMITTEE

27-28 May 1948

Medical Research Department
U. S. Submarine Base
New London, Connecticut

CLASSIFICATION CHANGED
TO ~~CONFIDENTIAL~~
AUTH Memo Ex Secy NRC Vision
DATE Committee 9 Dec 53
SECURITY OFFICER Frank B. Rogers Col MC

THIS DOCUMENT ~~RECLASSIFIED~~ DOWNGRADED
TO Unclassified
DHEW Secur. manual AUTHORITY Chapter 3-30 Paragraph 3-30-20C/3-30-40-A-1
BY J.B. Stewart DATE June 25, 74
Actg Phys. Sec. Officer, NIH

This document contains information affecting the national defense of the United States within the meaning of the Espionage Act, U. S. C. 50: 31 and 32. Its transmission or the revelation of its contents in any manner to an unauthorized person is prohibited by law.

~~CONFIDENTIAL~~

~~RESTRICTED~~

ARMED FORCES-NRC VISION COMMITTEE

Chairman: Dr. Richard G. Scobee
Deputy Chairman: Lt. Colonel E. N. Kirsten, USA
Executive Secretary: Donald G. Marquis
Technical Aide: H. Richard Blackwell

EXECUTIVE COMMITTEE

Official Members	Members at Large
Detlev W. Bronk	Conrad Berens
E. N. Kirsten	Arthur C. Hardy
C. W. Shilling	E. O. Hulburt
	Richard G. Scobee

MEMBERSHIP AND DISTRIBUTION LIST

	MEMBERS	ALTERNATES
AAF	Colonel Ben C. Holzman HQ, AAF, DC/AS, Research & Development Room 3D1079, The Pentagon Washington 25, D. C.	
		Dr. Paul M. Fitts Aero Medical Laboratory, MCREXD Psychology Branch Wright Field, Dayton, Ohio
ARMY AGF	Col. William Alexander Chief, F. A. Branch, 0-4632 Development Section HQ, Army Ground Forces Fort Monroe, Virginia	Col. Walter J. Klepinger Washington Deputy, AGF Room 4C-761, The Pentagon Washington 25, D.C.
		Colonel F. W. Farrell HQ, Army Ground Forces Fort Monroe, Virginia
AGO	Dr. Donald E. Baier Personnel Research Section, AGO Department of the Army, Room 1E880 The Pentagon, Washington, D. C.	Dr. Julius Uhlaner Personnel Research Section, AGO Department of the Army, Room 1D878 The Pentagon, Washington, D.C.
P&A Div. GSUSA	Colonel Andrew T. McAnsh Office, Asst. Chief of Staff G-1 Room 2C-930, The Pentagon Washington 25, D.C.	Major Jerome F. Lieblich Personnel & Administration Div., GSUSA Room 2C930, The Pentagon Washington, D. C.
Engrs	Mr. Noel W. Scott Engineer Board Fort Belvoir, Virginia	
Ord	Mr. John E. Darr Room 4C-430, The Pentagon Washington 25, D.C.	

	Members	Alternates
QMG	Colonel Jack E. Finks Office of the QM General Washington 25, D.C.	Mr. Arnold Court R & D Branch Office of the QM General Washington 25, D.C.
Signal Corps	Mr. Normand Stulman Special Projects Branch Engineer & Technical Service Office, Chief Signal Officer Room 3C-278, The Pentagon Washington, D. C.	
Research & Dev. Division	Colonel E. N. Kirsten Chief, Fire Control Design Division Ordnance Department Frankford Arsenal Philadelphia, Pennsylvania	
SG	Colonel Austin Lowery Jr. (MC) Officer in Charge, Eye Clinic Walter Reed General Hospital Army Medical Center Washington 12, D.C.	Colonel Don Longfellow Medical Research & Dev. Board Office of the Surgeon General The Pentagon, Washington, D.C.
NAVY		
BuAer	Lt. Comdr. Willard Davidson Bureau of Aeronautics Navy Department, Wash. D.C.	Mr. Leslie C. Mackrill Airborne Equipment Division Navigation Branch Room 1W53, BuAer, Navy Dept. Washington 25, D.C.
BuMed	Commander Harald A. Smedal Bureau of Medicine & Surgery Navy Department Washington 25, D.C.	
BuOrd	Mr. Michael Goldberg Bureau of Ordnance Navy Department Washington 25, D.C.	
BuPers	Captain F. B.C. Martin, USN Bureau of Naval Personnel Navy Department Washington 25, D.C.	Dr. Everett G. Brundage Bureau of Naval Personnel Navy Department Washington 25, D.C.
BuShips	Commander Dayton R. E. Brown Bureau of Ships Code 631, Navy Department Washington 25, D.C.	Captain H. T. Chase, Code 624 Bureau of Ships Navy Department Washington, D. C.
	Mr. C. S. Woodside Bureau of Ships, Section 921 Navy Department, Washington, D.C.	Mr. L.Nelson, Code 947, Room 3350 Bureau of Ships Navy Department, Washington, D.C.

	Members	Alternates
[N]RL	Dr. E. O. Hulburt Naval Research Laboratory Anacostia, Washington, D.C.	Dr. Richard Tousey Naval Research Laboratory Anacostia, Washington, D.C.
[O]NR	Captain C. W. Shilling Director, Medical Sciences Division Office of Naval Research Navy Department, Washington, D.C.	Dr. Henry A. Imus Head, Psychophysiology Branch Medical Sciences Division Office of Naval Research, Navy Dept. Washington, D.C.
[M]arine [C]orps	Major Walter L. Eddy, Jr. Room 2130, Division P & P Headquarters, USMC Washington 25, D.C.	

NATIONAL RESEARCH COUNCIL MEMBERS

Dr. Stanley S. Ballard Head, Department of Physics Tufts College Medford, Massachusetts	Dr. Conrad Berens Ophthalmological Foundation, Inc. 301 East Fourteenth St. New York, N. Y.
Dr. Detlev W. Bronk Johnson Foundation for Med. Physics School of Medicine University of Pennsylvania Philadelphia, Pennsylvania	Dr. George M. Byram Southeastern Forest Experiment Station Federal Building Asheville, North Carolina
Dr. Theodore Dunham Harvard Medical School 25 Shattuck Street Boston 15, Massachusetts	Dr. Clarence H. Graham Department of Psychology Columbia University New York, New York
Dr. Arthur C. Hardy Department of Physics Massachusetts Institute of Technology Cambridge 39, Massachusetts	Dr. H. K. Hartline Johnson Foundation for Med Physics School of Med. Univ. of Pennsylvania Philadelphia, Pennsylvania
Dr. Walter Miles Yale School of Medicine New Haven, Connecticut	Dr. Brian O'Brien Director, Institute of Optics University of Rochester Rochester 7, New York
Dr. Richard G. Scobee Department of Ophthalmology Washington University St. Louis, Missouri	Dr. Morris S. Viteles Department of Psychology 106 College Hall University of Pennsylvania Philadelphia, Pennsylvania
Dr. George Wald Biological Laboratories Harvard University Cambridge, Massachusetts	

LIAISON MEMBERS

U. S. Coast Guard
Commander R. T. Alexander
Chief, Testing & Development Div.
Coast Guard Headquarters
Washington, D. C.

National Bureau of Standards
Mr. C. A. Douglas
National Bureau of Standards
Washington, D. C.

Civil Aeronautics Administration
Dr. Barry G. King
Aviation Medical Service
Civil Aeronautics Admin., Wash., D.C.

Inter-Society Color Council
Dr. Deane B. Judd
National Bureau of Standards
Washington, D.C.

ASSOCIATE MEMBERS

Dr. Neil R. Bartlett
Psychology Dept.
Hobart College
Geneva, New York

Dr. S. Howard Bartley
Department of Psychology
Michigan State College
East Lansing, Michigan

Dr. Lloyd H. Beck
Department of Psychology
Yale University
New Haven, Connecticut

Dr. F. S. Brackett
Industrial Hygiene Research Lab.
National Institute of Health
Bethesda, Maryland

Mr. F. C. Breckenridge
National Bureau of Standards
Washington, D. C.

Dr. Alphonse Chapanis
Systems Research Project
Johns Hopkins University
Baltimore 18, Maryland

Dr. Howard S. Coleman
Department of Physics
University of Texas
Austin, Texas

Lt. Comdr. Ellsworth B. Cook
Medical Research Department
U. S. Submarine Base
New London, Connecticut

Dr. Forrest L. Dimmick
Medical Research Department
U.S. Submarine Base
New London, Connecticut

Dr. S. Q. Duntley
Room 4-306
Massachusetts Institute of Technology
Cambridge, Massachusetts

Lt. Comdr. Dean Farnsworth
Medical Research Department
U. S. Submarine Base
New London, Connecticut

Dr. Glenn A. Fry, Director
School of Optometry
The Ohio State University
Columbus 10, Ohio

Mr. W.C. Fisher
BuAer, Navy Dept.
Washington, D. C.

Dr. K. S. Gibson
National Bureau of Standards
Washington 25, D. C.

Dr. Irvine C. Gardner
National Bureau of Standards
Washington 25, D. C.

Dr. Walter Grether
Aero Medical Laboratory, MCREXD
Psychology Branch
Wright Field, Dayton, Ohio

Dr. Earl Green
Department of Zoology
The Ohio State University
Columbus 10, Ohio

Dr. LeGrand Hardy, Inst. of Ophthal.
The Presbyterian Hospital
635 West 165th Street
New York 32, New York

Dr. E. Parker Johnson
Bowdoin College
Brunswick, Maine

Mr. Harry J. Keegan
Division of Photometry & Colorimetry
National Bureau of Standards
Washington, D. C.

Dr. E. S. Lamar
Operations Evaluation Group
Navy Department
Washington 25, D. C.

Dr. Urner Liddel
Office of Naval Research, Navy Dept.
Washington, D. C.

Mr. A. Lovoff
Section 921
Bureau of Ships
Navy Department, Washington, D.C.

Dr. Carl W. Miller
Department of Physics
Brown University
Providence, Rhode Island

Dr. Hans Neuberger, Chief
Div. of Meteorology
Pennsylvania State College
State College, Pennsylvania

Lt. Harry Older
Aviation Psychology Branch
Bureau of Medicine and Surgery
Navy Department, Washington, D.C.

Dr. Gertrude Rand
The Institute of Ophthalmology
The Presbyterian Hospital
635 West 165th Street, New York, N.Y.

Mr. Louis P. Harrison
Chief, Technical Investigation Sec.
U. S. Weather Bureau
Washington, D. C.

Mr. Lawrence Karlin
AGO, Room 1E878, Dept. of the Army
The Pentagon, Washington, D. C.

Dr. John L. Kennedy
Research Lab. of Sensory Psychology
and Physiology
Tufts College, Medford, Massachusetts

Commander R. H. Lee
Naval Medical Research Institute
Bethesda, Maryland

Dr. Leonard C. Mead
Head, Human Engineering Section
Special Devices Center
Office of Naval Research
Port Washington, Long Island, N.Y.

Dr. Conrad G. Mueller
Department of Psychology
Columbia University
New York, New York

Dr. Kenneth N. Ogle
Div. of Physics & Biophysical Research
Mayo Clinic
Rochester, Minnesota

Dr. James C. Peskin
Vision Research Laboratory
304 W. Medical Bldg.
University of Michigan
Ann Arbor, Mich.

Mr. Nathan Pulling
Biological Laboratories
Harvard University
Cambridge, Massachusetts

Dr. Lorrin A. Riggs
Department of Psychology
Brown University
Providence, Rhode Island

Dr. William Rowland
Wilmer Ophthalmological Institute
The Johns Hopkins School of Medicine
Baltimore, Maryland

Major Lee Rostenberg
426 Port Road
Port Chester, New York

Dr. Charles Sheard
Div. of Physics & Biophysical Research
Mayo Foundation
Rochester, Minnesota

Dr. Clifford P. Seitz
Flight Section
Special Devices Center
Sands Point, Long Island, N. Y.

Mr. W. R. Sidle
BuAer, Navy Department
Washington, D. C.

Dr. Louise Sloan
Wilmer Ophthalmological Institute
The Johns Hopkins School of Medicine
Baltimore, Maryland

Dr. Jacinto Steinhardt
Operations Evaluation Group
Room 3827, Navy Department
Washington 25, D. C.

Dr. John Sulzman
1831 Fifth Avenue
Troy, New York

Dr. Joseph C. Tiffin
Department of Psychology
Purdue University
Lafayette, Indiana

Dr. W. S. Verplanck
Department of Psychology
Indiana University
Bloomington, Indiana

Dr. Robert Wherry
Department of Psychology
University of North Carolina
Chapel Hill, North Carolina

Dr. Herman S. Wigodsky
Committee on Atomic Casualties
National Research Council
Washington 25, D. C.

Dr. Benjamin J. Wolpaw
2323 Prospect Avenue
Cleveland, Ohio

ADDITIONAL DISTRIBUTION

Mr. C. L. Crouch
Illuminating Engineering Society
51 Madison Avenue
New York, New York

Dr. Alfred B. Focke
Sr. Scientific Consultant
U. S. Navy Electronics Lab.
San Diego, California

Surgeon Captain R. A. Graff
Royal Navy
British Admiralty Delegation
Washington, D. C.

Dr. William E. Kappauf
Department of Psychology
Princeton University
Princeton, New Jersey

Captain Wilbur E. Kellum (MC)
School of Aviation Medicine & Research
Naval Air Station
Pensacola, Florida

Dr. John Lentz
Secretary, NRC Committee on Ophth.
National Research Council
Washington, D. C.

Wing Commander J. H. Neal
British Joint Services Mission
1785 Massachusetts Avenue, N.W.
Washington, D. C.

Dr. T. W. Reese
Director, Psychophysical Research Unit
Dept. of Psychology and Education
Mount Holyoke College
South Hadley, Massachusetts

Dr. Arnold M. Small
Psychological Consultant
U. S. Navy Electronics Lab.
San Diego 52, California

Lt. H.G. Wagner (MC)
Aero Medical Equipment Laboratory
Naval Air Experimental Station
Naval Air Materiel Center
Philadelphia 12, Pennsylvania

Captain Thomas L. Willmon
Medical Research Dept.
U. S. Submarine Base
New London, Connecticut

Captain R. C. Young
Office of Naval Research
Navy Department
844 North Rush Street
Chicago, Illinois

Army Ground Forces Board No. 4
Fort Bliss, Texas

Director of Research
AAF School of Aviation Medicine
Randolph Field, Texas

Naval Medical Field Research Lab.
Camp Lejeune
New River, North Carolina

Office of the Assistant Naval
Attache for Research
Office of the Naval Attache
20 Grosvenor Square
London W1 England
Att: Medical Representative

Chief, Bureau of Medicine and Surgery
Attention:Div.of Aviation Medicine
Navy Dept., Potomac Annex
Washington 25, D. C.

Chief,Science and Technology Project
The Library of Congress
Washington 25, D. C.
Att: Mrs. H. L. Conn

Mr. W. E. K. Middleton
Physics Section
National Research Council
Ottawa, Canada

Lt. Comdr. C. F. Vance, USN
Optics Section Re4e
Bureau of Ordnance
Navy Dept., Washington 25, D.C.

Mr. M. O. Watson
Air Attache
Scientific Research Liaison Office
Australian Legation
Washington, D. C.

Dr. F. N. Woodward, Director
United Kingdom Scientific Mission
1785 Massachusetts Avenue, N.W.
Washington 6, D. C.

Army Ground Forces Board No. 1
Fort Bragg, North Carolina

Army Ground Forces Board No. 2
Fort Knox, Kentucky

Director, Research Activity
Bureau of Personnel
Navy Department
Washington, D. C.

Director, Systems Research Project
Johns Hopkins University
Baltimore, 18, Maryland

Training Library
Bureau of Naval Personnel
Navy Dept., Washington, D. C.

Aviation Psychology Branch
Air Medical Section
Headquarters Strategic Air Command
Andrews Field
Washington 20, D. C.

Major Anthony Tucker
Office of the Air Surgeon
Washington, D. C.

Dr. Duncan MacDonald
Optical Research Laboratory
Boston University
Boston, Massachusetts

ARMED FORCES-NRC VISION COMMITTEE

Minutes of the Twenty-first Meeting

May 27-28, 1948

Medical Research Department
U. S. Submarine Base
New London, Connecticut

The following were present:

AIR FORCES			Colonel Victor A. Byrnes Dr. Walter F. Grether Major R. A. Patterson
ARMY	AGO	(M) (A)	Dr. Donald E. Baier Dr. Julius Uhlaner
	Engrs	(M)	Mr. Noel W. Scott Mr. George W. Franks
	Signal Crops	(M)	Mr. Normand Stulman
	SG	(M) (A)	Colonel Austin Lowery Jr. (MC) Colonel Don Longfellow (MC)
NAVY	BuMed	(M)	Commander Harald A. Smedal
	BuOrd	(M)	Mr. Michael Goldberg
	BuShips	(M) (M)	Commander Dayton R. E. Brown Mr. C. S. Woodside
	NRL	(M) (A)	Dr. E. O. Hulburt Dr. Richard Tousey
	ONR	(M) (A)	Captain C. W. Shilling Dr. Henry A. Imus Dr. Everett F. Davis Mrs. Elizabeth Kelly
	Sub Base New London		Captain Thomas L. Willmon Lt. Commander Ellsworth B. Cook Dr. Forrest L. Dimmick Lt. Commander Dean Farnsworth Dr. Leon M. Rudolph Dr. Z. John Schoen
NRC MEMBERS			Dr. Stanley S. Ballard Dr. Conrad Berens Dr. Arthur C. Hardy Dr. H. K. Hartline Dr. Brian O'Brien Dr. Richard G. Scobee

LIAISON MEMBERS

Commander R. T. Alexander, U. S. Coast Guard
Mr. C. A. Douglas, National Bureau of Standards

ASSOCIATE MEMBERS

Dr. Neil R. Bartlett
Dr. Alphonse Chapanis
Dr. Howard S. Coleman
Dr. S. Q. Duntley
Dr. Glenn A. Fry
Dr. Earl Green
Dr. E. Parker Johnson
Dr. E. S. Lamar
Dr. Kenneth N. Ogle
Mr. Nathan Pulling
Dr. Gertrude Rand
Dr. Lorrin A. Riggs
Major Lee Rostenberg
Dr. W. M. Rowland
Mr. W. R. Sidle
Dr. Louise Sloan
Dr. John Sulzman
Dr. Joseph C. Tiffin
Dr. W. S. Verplanck
Dr. Benjamin J. Wolpaw

GUESTS

Miss Irene Blasdel, Naval Aviation Experiment Station, Franklin Institute, Philadelphia
Mr. C. L. Crouch, Illuminating Engineering Society
Dr. W. J. Crozier, Harvard University
Dr. David Freeman, Washington University
Dr. Douglas Fryer, New York University
Dr. J. J. Gibson, Smith College
Dr. James F. Kuntz
Mr. W. E. K. Middleton, National Research Council, Canada
Wing Commander J. H. Neal, British Joint Services Mission
Dr. Jesse Orlansky, Psychological Corporation
Dr. Robert B. Sleight, Purdue University
Dr. Arnold M. Small, Navy Electronics Laboratory, San Diego
Mr. C. R. Spealman, CAA, Dept. of Commerce
Lt. Commander M. Williams, U. S. Coast Guard Academy
Dr. Ernst Wolf, Wellesley College
Dr. Donald G. Marquis, Vision Committee Secretariat
Dr. H. Richard Blackwell, Vision Committee Secretariat
Mrs. Lucile E. Vauter, Vision Committee Secretariat

[redacted]

Thursday Morning, May 27, 1948   Page No

1. Captain Shilling called the meeting to order. He introduced Captain T. L. Willmon, Director of the Medical Research Laboratory, who welcomed the group to New London.

2. The Chairman called for corrections or additions to the Minutes & Proceedings of the 20th meeting. There were no corrections or additions from the floor.

   The Chairman requested that the following correction to the Minutes and Proceedings of the first meeting of the Subcommittee on Color Vision be recorded: Dr. Sloan has reported that on Page 6 the Minutes quote her as saying that the lantern was validated against questionnaires. Dr. Sloan states "I did mention the results of questionnaires because those data have never been reported, but I don't think they are of much value. The important validation studies were the performance tests with pyrotechnics and 'biscuit gun' signals. I did not emphasize these because they have been described in detail in several School of Aviation Medicine reports (Project 137, Reports 4, 5, 6, and 7)."

3. The Chairman requested that the Minutes and Proceedings of the meeting of the Subcommittee on Visual Standards, held May 26 at New London, be recorded. The Minutes of the Subcommittee meeting are recorded in the Proceedings - 13

4. Dr. Richard G. Scobee, Chairman of the Subcommittee on Visual Standards, presented a report of the Subcommittee meeting held May 26 at New London. An abstract of his report is contained in the Proceedings - - - 31

5. Lt. Comdr. Ellsworth B. Cook presented a paper entitled "Comparative performances of commercial screening devices and 20-foot wall charts utilizing the same visual test targets" - - - - - - - - - - - - - - - - - - 35

6. Dr. L. M. Rudolph presented a paper entitled "An experimental validation of checkerboard targets for measuring visual acuity" - - - - - - - - - - 53

7. Dr. Forest L. Dimmick presented a paper entitled "A study of dark adaptation with particular reference to method of testing night vision" - - - - 59

Thursday Afternoon,

8. Admiral James Fyfe, Commander of the Atlantic Submarine Fleet, welcomed the group to the Submarine Base, and introduced Captain Fenno, Commandant of the Submarine Base.

9. Dr. Conrad Berens discussed recommendations concerning the Dvorine method of retraining color blindness - 65

10. Dr. Henry A. Imus read the report of the Subcommittee on Color Vision - - - 67

11. Lt. Comdr. Dean Farnsworth presented a paper entitled "The use of the Navy Lantern in a coordinated color program for the Armed Forces" - - - - 68b

[redacted]

[redacted]

Page No.

12. Dr. Stanley S. Ballard commented upon specifications for sunglasses; a synopsis of his comments is contained in the Proceedings - - - - - - - 69

13. An election of members of the Executive Committee was held, and the following three new members were elected: Dr. Conrad Berens; Dr. Arthur C. Hardy; and Dr. E. O. Hulburt. Dr. Richard G. Scobee is the 4th elected member of the Executive Committee, serving the uncompleted term of Dr. Selig Hecht. Additional members of the Executive Committee are as follows: Lt. Col. Kirsten for the Army; Captain C. W. Shilling for the Navy; an Air Forces officer who has not been named as yet; and Dr. Detlev W. Bronk for the National Research Council.

14. A meeting of the Executive Committee was held Thursday evening. In accordance with the Procedures for Operation of the Committee, an election of Chairman of the Committee was held. Dr. Richard G. Scobee was elected to the Chairmanship for a two-year period. Lt. Col. Kirsten continues as Deputy Chairman until January 1949.

Official action has been taken by the Research and Development Board concerning the status of the Armed Forces-NRC Vision Committee. The Research & Development Board has ruled that they are willing to permit the Vision Committee to continue in its present operation since its function does not appear to overlap the function of the R&D Board. The official letter authorizing continuation of the Committee is in process at the present time. When the letter is received it is hoped that the financial support of the Committee can become evenly distributed among the three Armed Services which constitute the Committee.

The Executive Committee wishes to record its sincere appreciation of the valuable services Captain Shilling has rendered the Committee since its formation, in his capacity as Chairman. Because of his extremely active role in vitalizing the Committee, to many Captain Shilling *is* the Vision Committee. In electing Dr. Scobee to the Chairmanship, the Executive Committee felt that it was acknowledging its hopes for more active participation in the Committee by Army and Air Forces personnel. Captain Shilling's unique service to the Committee scarcely needs to be called to the attention of the Committee members. We are all happy that in his position as Liaison Officer for the Navy, Captain Shilling will continue to work for the best interests of the Committee and for the best interests of vision research in the Services.

At the meeting of the Executive Committee the following persons were approved for Associates of the Committee:

Captain J. T. Smith, USN
Lt. G. W. Rand, USN
Mr. E. Boghosian
Dr. David Freeman
Mrs. Elizabeth Kelly
Dr. Sherman Ross
Dr. William S. Carlson

[redacted]

[redacted]

Friday, May 28 Page No.

MINUTES AND PROCEEDINGS OF THE MEETING OF

THE SUBCOMMITTEE ON VISUAL STANDARDS

The Subcommittee on Visual Standards met on Wednesday, May 26, at New London, Connecticut. The following persons were present:

Dr. Richard G. Scobee, Chairman
Dr. Conrad Berens
Col. Victor A. Byrnes
Lt. Comdr. Ellsworth Cook
Lt. Comdr. Dean Farnsworth
Dr. David Freeman
Dr. Earl L. Green
Dr. Henry A. Imus
Wing Comdr. J. H. Neal
Major R. A. Patterson
Dr. W. M. Rowland
Dr. Louise Sloan
Dr. John H. Sulzman
Dr. Benjamin J. Wolpaw

The Chairman called the meeting to order. The Proceedings of the meeting follow:

I. VISUAL SCREENING DEVICE

Dr. Scobee reviewed the Subcommittee discussion and subsequent recommendation for a visual screening device, and reported that a contract for development of such an instrument is being arranged between ONR and Dr. Louise Sloan. An outline of Dr. Sloan's tentative plans was presented to members of the Subcommittee for review before the meeting. The outline is presented below, with discussion of the various questions raised.

PROPOSAL FOR FURTHER INVESTIGATION OF VISUAL SCREENING DEVICES

Louise L. Sloan, Ph.D.
Wilmer Ophthalmological Institute
Johns Hopkins University
Baltimore, Maryland

"At the request of Dr. Scobee, this rough outline of proposed plans has been prepared rather hurriedly in order to facilitate discussion of the problem at the meeting of the Sub-committee on Visual Standards. The present research project, for which the contract has not yet been signed, is still in the planning stage. We now have a Sight-screener and the latest model of the Telebinocular, which differs in a number of respects from that used in previous studies at New London, etc. The Ortho-rater has not yet arrived; consequently, our information about it is based primarily on previous reports. Tentative suggestions for further investigation are mostly in the form of questions, in the hope that those who have had more experience with these 3 instruments can give us valuable advice.

"In the following discussion the 3 instruments are designated as OR, SS

and TB. It is assumed that the machine test will be used to measure visual function in subjects not wearing any refractive correction, as well as in those whose refractive errors have been corrected by the necessary glasses.

TESTS OF VISUAL ACUITY

"The chief problems on which we would welcome discussion are as follows:

"(1) When monocular acuity is tested, should the other eye be excluded? In OR and TB the target viewed by the other eye contains considerable detail; consequently, in certain cases a significant difference may be expected in acuity, with and without occlusion. In SS the other eye views a plain white surface without detail. Results with and without occlusion, therefore, may be expected to show little or no difference.

"The testing of monocular acuity without occlusion of other eye (in OR and TB) admittedly tells us more about how the 2 eyes contribute to normal binocular vision than if each is tested with the other occluded. Cases in which there is some suppression of one eye in binocular vision, however, should be detectable by tests of depth perception, if these are adequate. In some statistical studies, no significant differences were noted in results with and without occlusion. It should be remembered, however, that as a rule, differences are to be expected only in subjects tested without refractive correction - and in this group, primarily in those with unequal refractive error and in only the non-dominant eye. In statistical studies which include a number of cases without significant uncorrected refractive error, a number with equal refractive error in the 2 eyes, the proportion of eyes in which suppression is a factor in binocular vision may be small. Marked differences in the results with and without occlusion may have occurred in a certain proportion of eyes without being apparent when data for the entire group with and without occlusion are compared. Data by Davis (J. Applied Psychol., 1946), obtained with OR, however, do show some difference with and without occlusion, even though the entire group of subjects is compared without further analysis.

"In view of above considerations, is the Committee willing to make a recommendation that acuity tests be made with occlusion, or do they recommend further study of this question to determine whether, for example, all such cases are detected by tests of depth perception?

"Our opinions regarding the advisability of occlusion are not in agreement with the point of view expressed in PRS Report No. 742, p 56. Slight negative loading on the "accommodation factor" are reported for the TB when a solid black field is presented to the non-tested eye. This, according to my interpretation, indicates a slight tendency for subjects with good near vision to have poor distance vision and vice versa. Cases of uncorrected myopia and of presbyopia would both be expected to show such an inverse relationship between acuity at near and at far. Provision of cues for the non-tested eye for "bringing the views of the 2 eyes into focus" (i.e. controlling accommodation of untested eye) might hinder rather than aid proper accommodation of the tested eye if there were a difference in refractive error in the two eyes."

Discussion

Dr. Wolpaw remarked that the only information he had on the subject came from a project reported by Commander Farnsworth. No significant difference was found between the results obtained on the Ortho-rater with both eyes and with occlusion of one eye.

Dr. Sloan remarked that she did not expect there to be a difference if the comparison was made utilizing the entire population. The difference would occur in the case of people with very different acuity in the two eyes. In these cases, occlusion of the weaker eye would improve acuity in the test eye.

Comdr. Farnsworth reported that Davis' data (op.cit.) reveal the basis for misleading results obtained on the entire population.) Individuals with poor acuity benefit by the use of the occluder. As acuity increases, the benefit becomes increasingly less until at high acuity there is a reversal.

Dr. Sloan felt that there was nothing to be gained by testing without occlusion. An estimate of the degree of binocular functioning can be obtained from a depth perception test.

Comdr. Farnsworth emphasized the small difference in acuity dependent upon the use or absence of occlusion, and suggested that for military purposes, the differences were ridiculously small.

Comdr. Cook mentioned the role of suppression, and reported that the use of the occluder can often be shown to clear up the sharpness of the target image.

Dr. Sloan asked whether there should be a test for suppression.

Comdr. Farnsworth stated his belief that a sensitive test should be developed.

Dr. Scobee asked Comdr. Farnsworth whether he would recommend, on the basis of his studies, that occlusion should be used in such a test.

Comdr. Farnsworth replied that first, an investigation should be made to determine whether there is such a thing as eye dominance that results in suppression.

Dr. Wolpaw remarked that the use of an occluder results in more consistent measurements with untrained examiners.

Dr. Scobee emphasized the necessity that the Subcommittee make a decision concerning the use of an occluder for testing monocular acuity. He emphasized Comdr. Farnsworth's remark concerning the relatively small differences involved. Dr. Scobee expressed a feeling that concern over small points might well retard the progress of the important developments.

Comdr. Cook suggested that the Subcommittee should consider the type of occluder to be used. He cited evidence that the acuity will differ depending upon the type of occluder and the way in which it is used.

It was the consensus of the group that it would be best to test monocular acuity with the other eye occluded, and that the occluder to be used should be a

translucent one, providing the untested eye with illumination equivalent to that of the eye under test.

"(2) Are tests of binocular acuity necessary, or may we assume that binocular acuity is equal to or very slightly better than the acuity of the better eye? This consideration may be important because, by eliminating tests of binocular acuity, space is provided for other purposes, e.g., for adding a test of color vision or for increasing items in other tests if this should prove desirable. An answer to this question can perhaps be obtained by further analysis of previously obtained data."

Discussion

Comdr. Farnsworth suggested that, usually, binocular acuity is slightly better than monocular acuity with the better eye, and asked whether members of the group were aware of exceptions to this rule.

Dr. Scobee reported that in some cases binocular acuity is poorer than monocular acuity in either eye.

Comdr. Farnsworth reported that the data obtained at New London seemed to indicate that, on the average, binocular acuity is better than the average of monocular acuities in the two eyes.

Comdr. Cook reported the exact data obtained in some detail, indicating significantly better acuity in the binocular case.

Dr. Wolpaw questioned the practical value of the additional testing required to obtain the binocular acuity measurement.

Comdr. Farnsworth suggested that approximately 1,000 cases had been studied at New London, the data from which could be re-analyzed to determine how many times the screening would have been inadequate had the binocular measurements been omitted.

"(3) A third problem concerns choice of type of target, i.e., checkerboard, discrimination of dots, lines, etc., Snellen letters, Landolt rings, illiterate E's etc. Advantages shown for checkerboard in previous studies are: high reproducibility, equal difficulty of targets of same size, and difficulty in memorizing correct answers. A possible disadvantage is that this type of target may overestimate acuity in subjects with uncorrected astigmatic errors of refraction. Are data already available which may be used to settle this question, or is further investigation indicated?"

Discussion

Dr. Scobee mentioned the AGO studies of visual test objects and the New London studies with which he assumed the members were familiar. He then reported his study in St. Louis, which is being conducted on 1200 to 1500 school children. The children are being tested on the Ortho-rater, the Telebinocular, the Sight-screener, and with the customary Snellen test. Dr. Scobee expressed his hope that the data would yield information concerning the relative desirability of the various test objects used.

Dr. Sloan remarked that the AGO and New London studies did not record the refractive error of the glasses which the patients were wearing.

Comdr. Farnsworth raised the question of the relation of astigmatism to acuity, as measured on checkerboard tests. He questioned whether the use of the checkerboard test permitted patients with astigmatism to attain visual acuities which they would not otherwise possess.

Dr. Rowland expressed his belief that astigmatism could aid patients in obtaining correct answers on the checkerboard charts.

Comdr. Farnsworth expressed concern over the magnitude of the error, feeling that it was probably a small effect.

Dr. Wolpaw questioned whether Dr. Scobee's study in St. Louis might be extended to answer this question.

Dr. Scobee remarked that at the present time 376 of the 1200 to 1500 patients have been tested, and that the St. Louis study should be completed by next February. He believed that it would be possible for analyses to be made to determine the degree to which astigmatic patients obtain better acuity on the checkerboard than with other test charts.

Dr. Scobee suggested that if it proved desirable, tests could be made with the checkerboard targets rotated to various axes.

"(4) A fourth problem concerns the gradation of steps in acuity. For the present, it may be necessary to choose these to agree, in part, with the present regulations on acuities required by various branches of the military services. In previous studies the criterion of normality of the distribution curve has been used in evaluating charts graded in equal steps on log scales, acuity scales, visual angle scales, etc. It should be remembered, however, that while corrected visual acuity is perhaps normally distributed if subjects with ocular pathology are excluded, a normal distribution is not to be expected in a group of subjects whose refractive errors are uncorrected. (See Table XX, PRS Report 742 p 70)."

Discussion

Dr. Sloan questioned the theoretical selection of the present Snellen visual acuity unit notation.

Dr. Scobee asked whether a roughly equivalent conversion table could be prepared which would correspond more closely to the facts of the distribution of acuity.

Dr. Wolpaw stated that it would be undesirable to change acuity notations because of the educational problem which would be encountered.

Dr. Scobee emphasized that the real problem was not the theoretical basis for the acuity measurement unit size, but rather the question of the interest of the Services in various possible cut-off scores.

Comdr. Farnsworth stated that the whole program conducted by AGO and at New London related to the percentage of the population exhibiting certain degrees of acuity. Comdr. Farnsworth recommended that gradation in acuity be made in terms of the percentage of the population possessing a given acuity. He suggested that the population be divided into 20 groups with equal numbers and that the mean acuity be determined for each of the groups.

Dr. Scobee asked whether such determinations could be made in the near future.

Comdr. Farnsworth reported that he believed such data could be obtained in the relatively near future.

Dr. Sloan pointed out that the problem of acuity measurement unit was important in itself, since comparison of the reliability of different acuity targets

cannot be made unless there is an equivalent number of steps in the tests.

Dr. Scobee summarized the discussion in the following way: "It appears that the present steps on the Ortho-rater are satisfactory for practical purposes. It is apparent that a more logical and scientific basis for selection of acuity units is desirable. Commander Farnsworth has suggested one method of approach to the problem which could be carried on separately, unless Dr. Sloan finds she has time to do it. We are agreed that it would be desirable to determine the mean acuities for various portions of the population." Dr. Scobee then asked whether the group would accept the present Ortho-rater scales as practical until such time as a supplementary study such as the one suggested by Farnsworth had been made. It was the consensus of opinion that Dr. Scobee's suggestion be accepted.

Col. Byrnes expressed his belief in the desirability of a 20/400 acuity letter for screening purposes.

It was agreed, therefore, that selection should be made in the acuity steps included in the acuity range from 20/50 to 20/15.

DEPTH PERCEPTION

"We are not in entire agreement with the point of view implied in PRS Report No. 742 (p 58) that form perception and "resistance to interference" are non-relevant factors in depth perception. The clue provided for the binocular discrimination of differences in depth is the disparity in the retinal images of right and left eyes. Ability to detect this disparity and interpret it as depth must vary with acuity. Poor depth discrimination is to be expected when one or both eyes has low acuity, or when there is a tendency toward suppression of either the right or left eye image in binocular vision. Such suppression is apparently one of the factors included under the term "resistance to interference".

"While it may be of theoretical interest to measure individual differences in depth perception with the factors of acuity and suppression held constant, for practical purposes in the Services, depth perception tests are now used (1) for the selection of stereoscopic range finder operators, etc. and (2) in the selection of air pilots, etc. In the second case there is some question as to whether motion parallax provides clues which are equal to or better than the binocular parallax clues used in judging the relative distance of stationary objects. Until it is definitely proven that binocular depth perception is not required in flying, some test for this function will be needed.

"One of the problems that arises in evaluating the 3 "machine" tests of depth perception is that while the reliability of the different tests is fairly good, the tests do not highly correlate with one another. In the absence of any data showing their correlation with some measure of depth perception whose validity is established, it is difficult to decide just what is being reliably measured.

"Among the possible reasons for the lack of agreement are these: (1) There are differences in the number of steps and range of parallactic angle covered by each test. (Smallest parallactic angle in SS is about 62% as great as smallest in TB). (2) Spurious high values of depth discrimination might be

obtained on the SS if the subject knows that the circle which stands out is always either No. 2, 3, or 4 and, therefore, has one chance in 3 of guessing correctly. In the OR and TB the chances of guessing correctly are 1 in 7 and 1 in 5, respectively. (3) The extremely large retinal disparities used in the upper ranges of the OR may be difficult for subjects with normal depth perception because of the conflict between the clues provided by accommodation and by retinal disparity. (This conflict is present in all such simulated depth differences, since no increase in accommodation is required for the apparently nearer object, but the discrepancy increases with increase in apparent depth).

"There is evidence in the literature (Langlands, for example) to show that many individuals fail tests in which depth differences are simulated in a stereoscope, but have normal depth perception when tested under conditions which do not introduce an artificial conflict between accommodation and convergence. This is immaterial if the screening tests are used to select sub jects for tasks involving judgment of stereoscopic depth, i.e., range finder operators, etc. If the Air Forces maintain the Howard-Dolman, or a similar test of depth perception, as a final criterion for aviation personnel, the machine tests may, nevertheless, be of value to them as a preliminary screening device if it can be shown that those who pass a test involving stereoscopic depth also pass the Howard-Dolman test. If so, only those who fail the machine test need be retested on the Howard-Dolman.

"It appears, therefore, that investigation should be made of possible modifications in the present depth tests, i.e., (1) the use of a preliminary demonstration such as that now provided with the SS to explain what the subject should see in the machine, (2) increase in the number of targets in the SS, (3) elimination of the larger depth differences in the OR, (4) extension of the range in the TB to include smaller parallactic angles, and possibly a change in the type of target, using identical figures instead of hearts, squares, etc. The results obtained on the machine tests should also be correlated with the results of tests using true rather than simulated depth differences - i.e., Howard-Dolman and Verhoeff tests, for example. (While the Howard-Dolman, as usually given, is not highly reliable, fairly reliable results can be obtained by a modification of the standard procedure). If the machine screening tests are to be used also in the selection of range finder operators etc., for validating studies, we should probably have some form of test which has been developed specifically for selection of such personnel. (Dr. Imus should be able to give some valuable advice on this point.)

Discussion

Dr. Sulzman asked whether the Armed Services were really interested in selecting for depth perception.

Dr. Rowland replied that in the case of stereoscopic range-finders, there was definite interest in depth perception.

Dr. Imus pointed out that visual acuity tests provide a preliminary screening for depth perception in that personnel with visual acuity as low as 20/30 are, in general, very poor at operation of range-finders.

Dr. Sloan asked whether tests of stereoscopic vision should not be validated against range-finder performance.

Dr. Imus replied by reporting results which had been obtained during the war

in which the Ortho-rater stereoscopic test was shown to be a valid predictor of successful range-finder performance.

Dr. Sloan pointed out the poor correlation between the stereoscopic tests in the Ortho-rater, the Sight-screener, and the Telebinocular. In view of the low correlation, Dr. Sloan asked whether it would be necessary to validate the Sight-screener and Telebinocular; it seemed obvious that only very poor validation could be obtained.

Comdr. Farnsworth warned against adopting the policy of including every test because each will add an additional datum. He emphasized the need for a careful evaluation of the gain to be expected by adding each test to the already extensive battery of tests.

Dr. Wolpaw asked whether it is possible for personnel to fail the Ortho-rater stereoscopic test and pass the Howard-Dolman depth perception test.

Dr. Imus replied in the affirmative.

Dr. Sloan remarked that personnel who passed the Ortho-rater do not need to be re-tested on other tests.

Dr. Imus asked whether the Subcommittee should consider including a test to prove the existence of adequate binocular coordination.

Dr. Scobee asked whether the Air Forces believed that depth perception was a critical ability for a pilot to possess.

Colonel Byrnes reported that he firmly believed that anyone flying an airplane should have good stereoscopic vision.

Dr. Berens remarked that he believed the speed factor was of prime importance in depth perception in flying. He stated his belief that a man can land a slow plane with good vision and without necessarily having good depth perception, but that landing a fast plane requires a more critical amount of stereoscopic vision.

Colonel Byrnes expressed his belief that if there were ever a need for mobilization again, it would be highly desirable to have complete visual profiles on all the personnel taken into the Armed Services. In this way, whenever a new requirement was initiated, it would be unnecessary to go back and test a new group of candidates. It would only be necessary to sort through the cards of individuals who had already been completely tested.

COLOR VISION

"The color vision test of the OR consists of an attempt to reproduce 4 charts from the Ishihara test. The TB uses forms composed of red and green lines in such a way that the red lines form one letter, the red and green lines, another. The subject is instructed to read the letter formed by red lines only. It is assumed that the color blind will not distinguish the red and green lines. The SS color vision test consists of a selection of pseudoisochromatic plates from the A. O. test.

"While we have as yet had no experience with any of these tests, on

inspection the construction of OR and TB tests appears faulty. It is probable that many color deficient subjects will pass these tests, and there is at present no evidence to show that those who pass have a milder degree of deficiency than those who fail. The use of a selected series of pseudo-isochromatic plates is in accord with the present procedures of the Navy and Air Forces. If this plan is followed, a source of artificial daylight illumination should be provided. Such a test will distinguish only between normal and color deficient subjects. If it is desired to qualify some of the latter group as having adequate color discrimination for certain tasks, further tests must be given to the 8% or so who fail the initial screening test, in order to determine their qualification for various assignments in which some degree of red-green color discrimination is necessary.

"Another possibility is to use a test which passes not only those with normal color vision but also a certain proportion of those with deficient color perception, i.e., those whose degree of defect is so slight that they can qualify for <u>any</u> service assignment. The Navy Lantern, for example, is passed by all normals and by about 25% of those with red-green color deficiency. If this 25% are acceptable as air pilots, and in all branches of Naval service in which color discrimination is important, then consideration should be given to the development of a test reproducing the cut-off point of the Navy Lantern for inclusion as part of the "machine" test. If such a test is adopted as the initial screening test, further testing of the 6% or so who fail may still be necessary, in order to identify those who meet the lower standards, such as that now specified in regulations as ability to "identify pure red and pure green", etc. If a pseudo-isochromatic plate test is used as the initial screening test, about 8% of those examined will fail and will require an additional test to determine degree of defect. If a test equivalent to the Navy Lantern is used, the number requiring further testing will be reduced from about 8 to about 6%. An additional argument in favor of the latter test is that memorization of the responses is less likely in a properly constructed test of the lantern type.

"Studies are planned which will determine the score on the Air Forces Color Threshold Tester which corresponds most closely to the cut-off point of the Navy Lantern. These will give some indication of the suitability of the standard Navy Lantern in the selection of air pilots. Studies are also planned to determine whether the essential features of the Navy Lantern can be reproduced as an integral part of the OR, SS, or TB. The OR and SS would appear to be the most suitable since they employ targets illuminated by transmitted light, whereas the TB targets are reflecting surfaces. In order to duplicate, at a distance of 14 inches, the visual angles subtended by the test lights of the Navy Lantern at 8 ft., the apertures must have a diameter of 0.01 inch and be separated by about 1/16 inch. Probably, there are practical difficulties in constructing such small apertures. However, larger point sources could be made, perhaps, to simulate in difficulty those of the Navy Lantern by decreasing the surface brightness of the test spot to maintain a constant candlepower. For example, test spots 1/16 inch in diameter could be used, with a surface brightness of about 26 ml. (before interposition of the colored filters). The angular separation of the two test lights, however, would have to be somewhat greater than in the Navy

<u>Discussion</u>

Dr. Sloan asked whether it would be possible to duplicate the essential features of the Navy Color Vision Lantern for a viewing distance of <u>1</u> foot. One of

the difficult engineering questions is the size of the aperture to be used. Actually, the aperture at a viewing distance of 1 foot would be only 0.01 inch, which would be too small to be practical. Dr. Sloan asked whether it wouldn't be possible to obtain the same candlepower for the sources of light rather than having the same visual angle.

Commander Farnsworth reported that it would be impossible to obtain the same brightness as in the Navy Lantern for use at 1 foot without revamping the whole machine.

Dr. Sloan asked whether it might not be better to utilize a selection of pseudo-isochromatic plates in a machine testing device.

Commander Farnsworth reported that even the use of pseudo-isochromatic plates involves complications unless another machine is built. Commander Farnsworth mentioned the difficulties in obtaining an adequate source of light for the plates.

There was some discussion of incorporating a color vision test into the present machines. Commander Farnsworth felt that it was more feasible to develop a separate machine for color vision testing, and reported that Special Devices Center had already been approached concerning such devices.

Dr. Wolpaw commented that operation of the screening test and the color vision test would double the number of personnel required to conduct the tests. He remarked that during the past war there were very few stations who had enough personnel for this kind of testing setup.

Dr. Imus suggested that at the present time the best procedure would be to produce the best possible set of pseudo-isochromatic plates, but that work should proceed toward development of a single slide for use in the machine screening device which would incorporate the principles of the Anomaloscope or Navy Lantern.

HETEROPHORIA

"The heterophoria tests appear to offer greater problems than do the other 3 tests of visual function. What we would really like to determine is whether or not the individual is likely to have asthenopic symptoms on prolonged use of the eyes under tiring conditions. In the practice of ophthalmology it is not usually necessary to make such predictions; the ophthalmologist has only to ask the patient. In setting qualifying standards for military service, however, it is necessary to make decisions as to the amounts of hyperophoria and esophoria or exophoria at far and at near which are likely to be associated with visual symptoms. Such decisions must be somewhat arbitrary. To quote Scobee (p 147), "The amount of hyperphoria is no indication of the amount of trouble it can cause". This is probably too extreme a statement, for it may be possible to set an upper limit above which visual symptoms will be present in a large per cent of patients, and a lower limit below which visual symptoms associated with the heterophoria are very unlikely. The qualifying amounts of heterophoria adopted for military services are based on experience gained with the usual clinical tests. It is principally for this reason that it is desirable to have the machine tests agree with the clinical tests. Agreement between clinical and machine tests requires not only a reasonably high correlation, but also agreement in mean values in prism

diopters for the different tests. For example, if machine test A showed a perfect correlation with clinical test A, but all measurements on the former were 2 prism diopters below those on the latter, then qualifying standards, in terms of prism diopters and based on experience with the clinical test, could not be applied to the machine test unless allowance was made for this constant difference of 2 prism diopters.

"The following conclusions regarding the different heterophoria tests are based, primarily, on the data reported by Sulzman, Cook and Bartlett, and in PRS Report No. 742, since we have had, as yet, no experience with the machine tests.

Reproducibility

"For each of the 4 types of phoria test (lateral, far and near; vertical, far and near) the reliabilities in terms of test-retest correlation are about equal in magnitude for the Maddox Rod and the 3 machine tests, and are, in general, higher for the lateral than for the vertical tests. However, in view of the fact that the size of the units employed is not the same throughout (1/2 pd for all vertical tests, 1 or 1 1/2 pd for lateral tests), and the fact that the range of vertical phorias normally encountered is much less than the range of lateral phorias, it is possible that the standard error of estimate gives a better comparative index of repeatability than does the test-retest correlation coefficient. In Table 1 the standard errors of estimate have been converted to prism diopters to permit comparison. It may be seen that, although the correlation coefficients are lower for measurements of vertical phoria, the errors of estimate range from 0.12 to 0.42 prism diopters as compared with values from 1.16 to 2.82 prism diopters for measurements of lateral phoria. Taking the data as a whole, the SS has the lowest errors of estimate of the 4 tests.

Table 1.

Standard Error of Retest Estimate in Prism Diopters
(Computed from scores and data relating scores
to equivalent values in prism diopters).

Lateral phoria at 20 ft.			At 13 inches		
	S.E.E.	r		S.E.E.	r
MR (from Table I)	1.18	0.87	(Table I)	3.33	0.74
Screen MR (Table I)	1.43	0.89	"	2.25	0.82
Screen parallax (Table I)	1.79	0.72	"	4.14	0.67
MR (Table IV)	2.37	0.79	(Table V)	2.82	0.87

	Lateral phoria at 20 ft.		At 13 inches		
	S.E.E.	r		S.E.E.	r
OR (Table IV)	1.22	0.87	(Table V)	1.55	0.92
SS " "	1.16	0.80	"	1.65	0.83
TB " "	2.35	0.76	"	2.59	0.88

Vertical phoria

	At 20 ft.		At 13 inches		
	S.E.E.	r		S.E.E.	r
MR (Table IV)	0.30	0.62	(Table V)	0.42	0.68
OR " "	0.39	0.63		0.325	0.62
SS " "	0.115	0.61		0.165	0.55
TB " "	0.305	0.60		---	---

"A second consideration in evaluating the repeatability of phoria measurements involves changes in the mean score in a second test. These are quite small in many cases, but surprisingly large in others. (See Table 2). The OR and the MR both show, in some instances, changes in mean score much greater than those for the SS and TB.

Validity

"If we assume that the purpose of phoria tests is to distinguish individuals likely to have asthenopic visual symptoms, then the validity of the measurements provided by the different tests should be determined, ideally, by some independent study of the degree of visual discomfort experienced by the subject. In the absence of any such specific information we may accept some standard clinical phoria test as the validating test. There are at least two arguments for accepting the clinical test as a criterion by which to judge the machine tests. (1) In the clinical phoria tests, for a distance of 20 ft., the subject must accommodate for 20 ft. if the target is to be clearly focussed. When the phoria measures zero, his visual axes are converged upon the target at 20 ft. The machine tests for "optical" distance are assumed to reproduce these accommodation and convergence requirements by means of lenses and prisms. Even if examination of the optical system verifies this assumption, the optical illusion is not perfect, and the subject may overconverge because certain clues lead him to believe that the optical images of the targets are nearer. At near, however, this difference in the clinical and machine tests is not present in the case of the SS. The SS differs from the Maddox rod test at near only in the method used to present different images to right and left eyes.

Table 2.

Instances of Relatively Large Differences in Test Mean and Retest Mean
(E = esophoria X = exophoria)

	Test Mean	Retest Mean	Difference	r	
20 ft. Screen and parallax	E 1.42	E 0.47	0.95	0.72	(Table I)
" " OR Excursion	X 4.19	X 2.95	1.24	0.76	(Table VII)
" " OR No. 1 Standard	X 4.12	X 2.77	1.35	0.92	" "
" " OR No. 2 Standard[1]	X 1.69	X 0.73	0.96	0.86	" "
" " MR	LH 0.08	RH 0.28	0.36	0.62	(Table IV)
13 inches MR	X 3.87	X 4.99	1.12	0.74	(Table I)
" " MR	X 3.84	X 3.04	0.80	0.87	(Table V)
" " OR	X 4.12	X 3.27	0.85	0.92	" "
" " Screen MR	X 4.24	X 5.40	1.16	0.82	(Table I)

[1]Note marked difference in means with 2 different instruments. Indicates some difference in construction of 2 instruments?

"The optical systems of the OR and TB should be investigated to determine the actual accommodation and convergence requirements prevailing in the near tests. (2) A second very practical reason for accepting some clinical test such as the Maddox rod test as the criterion, is that decisions as to what degrees of phoria should disqualify are based on clinical experience with such tests.

"At lease one argument may be advanced, however, that the machine tests may actually offer a more valid measure of phoria than the Maddox rod test. The purpose of phoria tests at 20 ft., or at 14 inches, is to measure the angle of convergence of the visual axes when the eyes are accommodated for 20 ft., or for 14 inches, and fusion impulses are removed by presenting dissimilar images to the two eyes. The machine tests may provide a more adequate control of accommodation for 20 ft., or for 14 inches, than the Maddox rod test, because the test targets in the former case consist of figures, dots, arrows, etc; whereas, in the Maddox rod test the targets are a circular spot of light and a line.

"Intercorrelations between the 4 tests show that the 3 machine tests, in general, correlate more highly with one another than they do with the Maddox rod test. It is not possible to decide from these data whether the low correlation with the clinical test is to be attributed to the inadequate simulation of the far and near distances by the machine tests, or to inadequate control of

the accommodation factor by the Maddox rod test, or to other factors. In the case of the SS at near, there is no question of inadequate simulation of the 14 inch distance; nevertheless, the correlation with the Maddox rod test is only 0.54. This suggests that poor control of the accommodation factor by the Maddox rod test is in part responsible for the low correlations. It is interesting that while the correlation of OR and MR for lateral phoria at distance is only 0.56, Wirt (J. Applied Psychol., 1943) obtained correlations of 0.76-0.84 with the OR, and with a different type of phoria test at an actual distance of 20 ft. The latter was a prism diplopia test in which phoria was measured by the alignment of an arrow seen by one eye with a scale seen by the other eye."

Discussion

Commander Cook asked whether there was any evidence of the degree of variation in phoria as a function of time.

Dr. Scobee reported that he was not aware of any studies in which the same individual was tested repeatedly in time.

Dr. Berens emphasized that the measurement of phoria in time would be dependent upon the activity of the eyes between measurements.

Problems which need further study.

"(1) Investigation of possible causes of disagreement between the results of the 3 machine tests. In view of the fact that the test targets are essentially similar in all three instruments, it seems likely that differences in the optical systems, or differences in procedure, or both, may account for the relatively low correlations. It has been pointed out by Wirt that when the phoria tests are immediately preceded by tests requiring fusion, i.e., tests of depth perception or of binocular acuity, (as is the case in the SS) the findings are changed in the direction of increased exophoria and decreased dispersion of results.

"(2) Investigation of causes of disagreement between "clinical" tests and "machine" tests. Possible factors are the simulation of distance by optical devices, poor control of accommodation in Maddox rod test, etc.

"(3) There is need for fundamental investigation to find whether some test or group of tests of the function of the oculorotary muscles will provide a basis for reliable predictions as to whether or not asthenopic symptoms are likely to occur. Without such information there is no definite basis for evaluating the usefulness of any of the simpler tests for heterophoria. The most constructive suggestion we can make in regard to the particular problem is, "let Scobee do it".

"(4) In order to meet the immediate needs of the military services, it might be well to find out whether the situation is really as bad as it looks, and whether, in spite of the low correlations, some one of the machine tests is, perhaps, fairly satisfactory as a preliminary screening test. It may be possible, for example, to distinguish 3 classes, i.e., (a) those with suppression or with obviously disqualifying degrees of heterophoria (b) those whose phorias are definitely within normal limits and (c) a borderline group which may require further examination with other types of test."

II. VISUAL ACUITY TRAINING PROGRAM

Dr. Scobee called for a discussion of the proposal submitted by Dr. Conrad Berens for a study of visual acuity training. Dr. Berens submitted a summary of his project plans which was circulated to members of the Subcommittee prior to the meeting. The summary is presented below, with discussion of the proposal.

SUMMARY OF VISUAL ACUITY PROJECT

Conrad Berens, M.D.
Director, American Committee on Optics and Visual Physiology

1. Purpose - To determine the influence of group training in visual form perception upon the uncorrected and corrected visual acuity.

2. Background - At the meeting of the American Committee on Optics and Visual Physiology in June, 1944, it was recommended that the effects of visual training upon visual acuity should be determined independently in three different places.

   Important investigations on this subject were being carried out at that time at Johns Hopkins and Washington University. Briefly, these two studies concluded that while the ametropia was not altered significantly by the various methods of visual training employed, nevertheless, there was an improvement in the uncorrected visual acuity in about 25% of the subjects.

3. New Data - Investigations which the proposed study seeks to accomplish and which were not covered in the two previous studies are as follows:

   a. The significance of one specific type of training, i.e., that purporting to to improve visual perception of form, rather than a combination of the above with techniques directed toward altering the neuromuscular factors (accommodative-convergence relationship, rotations, vergences, etc.)

   b. The practicability of training on a group basis rather than an individual basis.

   c. The importance of Controls. One group will receive training and wear glasses. These will be matched with a group who receive training and who do not wear glasses. A third group will not receive training and will not wear their glasses, and a fourth group will receive no training and will wear their glasses.

   d. The use of visual acuity tests (checkerboard) and illumination approved by the Army-Navy-NRC Vision Committee.

   e. The influence of test chart exposure on visual acuity.

4. Importance - This study should give answers to the following:

   a. The possibility of isolating the cause of improvement in visual acuity as found in the two previous studies.

   b. The possibility of influencing visual acuity by group training with a relatively simple technique.

c. The possibility of providing significant data which may aid in the visual selection procedures of the military services.

d. The possibility of improving the visual performance of fire control personnel, in ship and plane recognition, etc.

e. The possibility of providing data on the overall myopia problem, viz., the stability of myopia, the visual habits of young myopes, the inter-relationship between visual perception and personality organization.

f. The results of testing visual acuity by clinical methods and screening instruments.

Discussion

Dr. Berens commented on the importance of the existence of visual acuity training in connection with setting visual standards for use by the Armed Services. Obviously, if it is possible for visual acuity to be trained, many candidates with training will be admitted, whereas candidates with equivalent refractive condition might be rejected. In addition, Dr. Berens pointed out that it is desirable to advise the public concerning the degree to which visual training is possible. Dr. Berens requested the Subcommittee to consider the proposal and stated that he would welcome suggestions.

Dr. Sloan remarked that one valuable contribution of the program could be to discover to what degree the improvement in acuity resulting from training transfers to relatively unfamiliar objects, such as aircraft. She stated her belief that the training program results in significant improvement in the interpretation of letters, but that perhaps transfer to other objects was not possible.

Dr. Sloan stated further that the largest improvement probably occurs in cases of astigmatism. She suggested, therefore, that Dr. Berens' study might be expanded to include careful study of astigmatism as well as simple myopia.

Dr. Berens requested that the Subcommittee comment on the type of training which might be used. He stated that consideration had been given to two or three different methods, including the tachistoscopic method of Renshaw. There was considerable discussion, but no definite decisions were made concerning the method to be used.

Dr. Imus asked to what degree intelligence would be measured. He felt that the ability to respond to small cues resulting from blurred images might be correlated with intelligence and motivation.

Dr. Berens agreed that it was highly desirable to control or measure both intelligence and motivation.

Dr. Berens raised for discussion the question of the type of test object to be used in the training. He stated that he had thought of using the checkerboard test, such as is included in the Ortho-rater.

Dr. Sloan remarked that it is possible to train patients with astigmatism to perform well on the checkerboard type target, although the increase in performance does not transfer to other kinds of visual tasks.

There was considerable discussion of the desirability of using various targets, but no definite decision was reached.

Commander Farnsworth stated that he was interested in isolating the factors effective in training visual acuity so that definitely better methods of training might be developed.

III. VISUAL ACUITY TESTING MANUAL

The Manual for Testing Visual Acuity, prepared by the Subcommittee last year, was reviewed by the Subcommittee. Commander Farnsworth had called attention to the fact that the brightness specification for standard visual testing was stated as 15 to 25 footlamberts in the manual, whereas he believed the Subcommittee had agreed upon 10 to 15 footlamberts. It was discovered that in the correspondence the figures had become confused; hence, the manual had been prepared in error. It was agreed that the manual should be corrected and notification made to the Surgeons General that the manuals originally submitted were in error to this extent.

REPORT OF THE SUBCOMMITTEE ON VISUAL STANDARDS

Richard G. Scobee, Chairman

The Subcommittee on Visual Standards met on May 26, 1948, at New London, Connecticut. The chief item on the agenda was discussion of a visual screening device.

At a previous meeting of the Committee, in August 1947, the general problem of visual testing was considered, and a recommendation was made that a machine screening device be used for initial visual examinations in the Armed Services. This recommendation was formally submitted to the Surgeons General of the various Armed Services. A reply was received from the Air Surgeon indicating his interest in the recommendation, and requesting that the Subcommittee make more specific recommendations about the particular type of screening device to be used. This matter received considerable attention at the meeting of the Subcommittee at New London.

The discussion of a screening device was focused by Dr. Louise Sloan, who has submitted a proposal to the Office of Naval Research for an investigation of visual screening devices with the ultimate objective of developing a new device, or of modifying some of the existing devices for military use. Prior to the meeting of the Subcommittee, Dr. Sloan had examined the literature and had submitted a general plan of her proposed research.

Dr. Sloan's proposal and the discussion of her proposal are recorded in the Minutes of the Subcommittee meeting, which are included in the Proceedings of the Vision Committee meeting. The questions she raised and the conclusions reached by the Subcommittee will be only summarized here.

Status of Untested Eye

The first question raised concerns the status of the eye not under test when monocular acuity is being tested. The untested eye could be exposed to a plain white field, a plain black field, or a field approximately equivalent to the field viewed by the eye under test. For example, in the Ortho-rater, the untested eye views a checkboard pattern equivalent to the pattern viewed by the eye under test, except for the absence of a differentiated square.

It was the opinion of the Subcommittee that the eye under test should be occluded during tests of monocular acuity. The existence of suppression in some, and resistance of unequaled accommodation in others, makes occlusion seem highly desirable. Because of binocular effects of pupil size and illumination in the untested eye, both acting as variables on the tested eye, it is recommended that the occluded eye receive transmitted illumination approximately equal to the illumination of the tested eye.

Tests of Binocular Vision

The next question raised was whether it is necessary to test binocular, in addition to monocular, acuity. The Subcommittee agreed that testing binocular acuity is not necessary. There are very few situations in which visual

acuity with both eyes is found to be worse than visual acuity with the better eye. In these few situations, the visual difficulty can usually be detected by other portions of the visual examination. Hyperphoria, for example, may produce worse binocular than monocular acuity, but hyperphoria will be detected in examinations of muscle balance.

Commander Farnsworth has mentioned that large numbers of records are available at New London in which both monocular and binocular acuity were measured. Commander Farnsworth suggested that he could examine the data to ascertain whether the omission of binocular acuity scores produced differences in visual acuity rejections.

Type of Test Object

It was recognized that the recent studies conducted by the Army AGO demonstrated certain advantages of the checkerboard test objects, such as high reliability, equivalence of item difficulty, and freedom from malingering. Dr. Sloan commented on the possible disadvantage of this type object in that persons with astigmatism obtained better scores with equivalent acuity than persons with simple spherical errors. A study of 376 school children is being conducted at St. Louis (by Dr. Scobee), utilizing the three commercially available screening devices. Refractions and visual acuity measurements are available. Dr. Scobee agreed to re-analyze his data in order to ascertain the influence of astigmatism upon visual acuity tested on the checkerboard targets. If analysis of the data indicates a significant effect, Dr. Scobee suggested that the checkerboard targets could be rotated and acuity measurements made in the case of patients exhibiting astigmatism.

Visual Acuity Units

The next question raised for discussion concerned the most desirable visual acuity scale. Alternative possibilities, such as the Snellen type notation, the constant increment increase in visual angle, or the logarithmic scale of visual angle, were considered. The Subcommittee did not consider that data were available which would permit a definitive answer. Tentatively, it was agreed that the present Snellen notation be used, but that only certain steps be used in accordance with the interest of the Armed Forces. The military services are not interested in acuity below 20/400. They need to differentiate between 20/400, 20/200, 20/100, and 20/50. Between 20/50 and 20/15 an intensive scale is needed. The use of irregular increments in the Snellen notation is recommended for military use until further evidence justifies a change.

It was suggested by the Subcommittee that it is of primary importance to determine the population distribution of visual acuity. Apparently, the Armed Forces are not willing to undertake job analyses to determine the visual requirements of various military tasks because of the excessive cost. They are satisfied to estimate the approximate number of men available who possess various degrees of visual acuity. For this purpose, then, it would be desirable to divide the population into 20 groups, each containing 5% of the total population, and to determine the mean visual acuity for each group. Such a study would yield an index of the men available for total mobilization, utilizing various cut-off scores. It was suggested that the large num-

ber of Ortho-rater test scores already available in industries and elsewhere would provide a reasonable estimate of such population data.

Depth Perception

When the general topic of depth perception was raised for discussion, it became apparent that there are at present no definite criteria for validating depth perception tests. Fortunately, it was the belief of the Subcommittee that depth perception is not particularly important in the large majority of military jobs. In addition, there is a fair assumption that good binocular acuity is correlated with good depth perception. It is encouraging that Dr. Imus has shown the Ortho-rater stereoscopic test to be correlated with successful range-finder performance. What has not been done is to correlate the stereoscopic Ortho-rater test with tests of depth perception made at actual distances. The Air Force desires knowledge of depth perception tests other than the machine tests, for use in selecting prospective pilots. Until a better test has been developed and validated, the Air Force will continue to use the Howard-Dolman test. The Subcommittee wishes to comment that from the standpoint of testing large groups of men, a machine stereoscopic test is probably more feasible, and may be assumed to have some validity for other military jobs than those for which it has been validated.

Color Vision

Present data indicate that the Navy color vision lantern is a very good test. It would be desirable to include a test of color vision in the machine screening device. The possibility of including the Navy lantern in a machine test involves difficulties of an engineering nature. For example, the size of holes necessary at near viewing distances would be prohibitively small. One difficulty with the present color vision tests in the screening devices is the illumination on the pseudo-isochromatic plates. It was agreed by the Subcommittee that if one of the better sets of pseudo-isochromatic plates were used, and if adequate illumination could be provided, such a machine test might provide the best answers available at the present time.

The Subcommittee emphasized the importance of the use of a continuous scale of color vision abilities, since color vision standards may change at any moment. Presumably, this aspect of color vision testing could well receive greater attention.

Tests of Heterophoria

The Subcommittee suggests that in present machine tests of heterophoria cheating is possible. In many installations, the examinees discover in advance where the zero point is on the vertical scale. This difficulty was circumvented at New London by shifting the zero point at some times but not others, or by inserting known amounts of prism, differing from test to test.

The chief question that arises in heterophoria testing is the clinical significance of the findings. It is obvious that no single measurement in muscle balance is valuable by itself, but that all measurements must be interpreted together. Therefore, it would appear that a population study

should be made on heterophoria, and arbitrary cut-offs established for use in machine tests. Examinees who fall outside the cut-off limit would then be sent to a more complete medical examination.

One question was brought before the Subcommittee on which an answer was not attempted. This concerned the recommendation of definite charts for use in visual acuity testing. The Subcommittee felt that this matter should receive very careful attention, and that no answer should be given at the present time.

Another item which was discussed by the Subcommittee was a proposal made by Dr. Berens for a visual training study, which at the moment concerns myopes. Two studies have been conducted in the past, at Johns Hopkins, and at Washington University, which showed no changes in the refractive state of the eye during the training period. It was also shown that visual acuity may improve, or the patient may think his acuity has improved, although measurements do not confirm his feeling. In each study, approximately 25% of the population tested showed either demonstrable improvement or sensed improvement. Dr. Berens' proposal is to re-study the problem, using more careful controls than have been previously used. The Subcommittee considered a number of aspects of his study, such as test objects, intelligence factor, and motivational factors. The Subcommittee raised the question whether it would be possible to include follow-ups in the study. The Subcommittee would very much like to know, as a point of academic interest, why some patients seem to improve after training. The degree to which divorcement of accommodation and convergence improves acuity, and the role of training for normal sub-threshold cues should be discovered.

Discussion

Dr. Berens requested comments from the Vision Committee concerning the type of test object to use during training.

Dr. Marquis suggested training acuity on one type chart, then testing acuity on the same type chart and on a different type of test object. He felt that in this way the degree to which the training was specific would be uncovered.

Dr. Scobee remarked that in the St. Louis study, improvement in acuity occurred in those individuals who originally possessed sub-standard visual acuity for their refractive error.

Dr. Hartline remarked that the improvement in acuity was not large, and occurred in a relatively small portion of the population. He asked Dr. Berens how enthusiastic he was to study this effect.

Dr. Berens replied that he felt it was important for visual acuity training to be evaluated since a relatively large amount of money is being spent on this training by ordinary citizens. He remarked further that, just as color vision training received a strong scrutiny, visual acuity training should receive the same scrutiny, so that the public would not be swindled by what might turn out to be fraudulent claims.

Dr. Fry suggested that the difficulty with checkerboard targets which arises from patients with astigmatism could be overcome by rotating the checkerboard test objects through 45°.

COMPARATIVE PERFORMANCE OF COMMERCIAL SCREENING DEVICES AND FAR AND NEAR WALL CHARTS UTILIZING THE SAME TEST TARGETS.

E. B. Cook, Lt. Cdr. (MSC) USN

Introduction:

A previous study of the comparative measures obtained with three commercial screening devices (the Bausch and Lomb Company's Ortho-Rater, the American Optical Company's Sight Screener and the Keystone View Company's Telebinocular) and by means of the 20-foot clinical method of testing was conducted at this activity, and a report on the test-retest reliability of the scores obtained was made to this committee.* The Personnel Research Section of the Adjutant General's Office requested the use of our data for further processing as part of a broad study of visual acuity. Results of their factor analysis of Medical Research data were presented to the committee by Dr. Robert Wherry **, and published as PRS report No. 742, August 1947.

This factor analysis revealed the presence of several independent factors which played a definite role in the variance of test scores obtained. The main ones were identified by AGO as retinal resolution, lens accommodation, form (letter) perception, depth perception, and resistance to interference, this last being definitely an instrument factor.

Results of the first statistical treatment showed that the scores obtained by the commercial devices and with the wall charts were quite reliable in a test-retest situation. However, the results of the factor analysis study indicated that non-pertinent factors accounted for a sizable portion of test score variance, the percentage varying with each instrument and method. There is no point in dwelling on specific examples here, but the whole study emphasized that while all the tests are on the whole reasonably reliable, mere reliability must not be mistaken for validity, and that we cannot assume interchangeability of either interpolation of scores or validities from one machine or method to another. What is measured is measured well but apparently much of what is measured is not relevant.

As an intitial step in determining whether the differences in scores obtained with screening devices and by clinical methods were attributable to the instruments per se or to the different test objects used, the subject experiment was devised, utilizing the instrument targets in visual alleys in addition to the letter charts customarily employed in clinical methods. Manufacturers of the screening devices were asked to reproduce their machine test targets for use at the 20-foot distance and the 13-inch near point distance with the same visual angle subtended on the wall charts as on the machine charts. Although the companies willingly cooperated in this, the photographic reproductions for the most part were not entirely satisfactory. However, negotiations on procurement were so extended and time-consuming that we finally selected the best from those submitted and decided to use them if the experiment was ever to be initiated.

- - - - - - - - - - - - -

* Minutes and Proceedings of the 15th Meeting of the Army-Navy-NRC Vision Committee, February 1946.

** Minutes and Proceedings of the 20th Meeting of the Army-Navy-NRC Vision Committee, October 1947.

Procedure:

The New London chart was selected as the standard test against which the comparative performances of the other tests would be evaluated. The New London chart is constructed of letters according to Snellen size specifications but the selection of letters is on the basis of equality of "seeing" difficulty, and there are an equal number of letters in the "critical" testing lines. The New London chart has, then, more grades of acuity at the critical level and the finer distinctions result in a better distribution of scores.

Testing procedures and illumination of the testing areas were in conformity with the standards established by the SubCommittee on Visual Testing. The tests were conducted in various screened compartments of a large well-lighted and well-ventilated room. Before and after each testing sequence, the illumination in each testing compartment was checked with a photometer.

The population for this study was composed of naval personnel (enlisted man, chief petty officers, Waves and nurses), male and female civil service workers, and male high school seniors. The hetereogeneity in visual ability of this population is approximately comparable to that of a cross-section of a population which might be examined by a national military selective service, but is probably more homogeneous than an ophthalmologist would expect to find in civilian practice because of the heavy loading of the sample with young men.

In all, 128 observers were measured without correction in a test-retest situation. Subjects were tested in groups of eight and the experimental procedure allowed two such groups to be tested each morning and retested the afternoon of the same day. The order of tests was randomized from person to person by the Latin Square technique. No one was informed of his or her score, and all were assured that the results would have no bearing on subsequent service or duty status.

Brief medical histories were obtained from each subject prior to testing to ensure that their scores would not reflect transient effects as those resulting from long periods of travel, lack of sleep, over-indulgence in alcohol, or the use of drugs like the sulfa derivatives which may give rise to temporary visual disturbances.

Prior to the testing session, a short talk was given to each group in order to explain briefly the purpose of the experiment, the test procedure and the importance of rigid adherence to the test sequence.

Eight full-time operators were carefully instructed in standardized test procedures in practice sessions prior to the running of the experiment.

Scoring procedures followed the methods set up in our earlier study: the score assigned on the Ortho-Rater and Telebinocular was the last correct response called before two consecutive errors, and on the Sight Screener two errors in any test line indicated the score as the line previous to this. Prior to the running of this experiment, the Keystone View Company indicated its preference for another testing procedure with the Telebinocular, and this was included in the study. In the Telebinocular test targets the task is to discriminate between dots, lines and solid grey circles in a series of 22 test items of increasing difficulty. Although the subject is required to give a response for the entire 22 items, the company method of scoring is concerned only with the response given to thirteen

significant test items interspersed throughout the series, and the score assigned is the last consecutive correct response on the significant items.

With the exception of the Telebinocular, each other instrument and corresponding alley test utilized the same test targets at near and far. The Telebinocular near "circles" test is a form discrimination task, whereas its far acuity test is a dot resolution one. Since no comparable far "circles" test was available, data is presented for the Telebinocular only on near acuity.

Two Ortho-Raters are available at this activity, and in our earlier investigation these were used with test slides carefully matched for density value to determine how effectively different instruments of the same type measured the same functions. Almost identical reliabilities, validities, means and variabilities were obtained for the two on acuity data but not for phoria scores. In this experiment, available slides were carefully scrutinized and the two sets exhibiting the greatest difference in density were employed in the two instruments. This appears to have had no effect on the resultant scores obtained, nor should any be expected since the density tolerance limits are doubtless carefully controlled by the manufacturer.

RESULTS

Introduction:

Results are discussed under two sections. The first is concerned with the means, standard deviations, reliability and validity coefficients of the various test-retest scores; the second involved various tests of significance which were applied to the data.

Frequency distributions for the scores obtained on all tests for right eye, left eye and binocular have been mimeographed for reference. Time does not permit a discussion of them here.

Section I:

The means, standard deviations, reliability coefficients and validity coefficients for the three commercial devices, with their corresponding alley targets and for the New London Letters are presented in Tables I through IV.

Note the two methods of Telebinocular scoring. Laboratory scores are indicated as the A score, and the score obtained by company method is presented as the B score. The company score represents the acuity score for each subject in decimal equivalents of Snellen fractions. The equations for converting A scores to B scores were determined graphically and are presented in a footnote for each table involving Telebinocular results.

Reference to Tables I through IV indicate that the reliabilities are all reasonably significant. The reliabilities are somewhat lower than those obtained in our previous study using the commercial devices and the Snellen and New London Letter conventional alley targets. This may be a reflection of the fact that from the visual acuity standpoint, the population used in this project was more homogeneous. In contrast to this general downward trend, the New London Letters have a higher reliability than attained in the previous experiment.

On the average, the retest means are generally slightly higher than the test means. This is probably the result of a mild practice effect.

Test-retest standard deviations are about the same, but those of the Sight Screener are consistently higher on retest as are also the means for this instrument. Apparently there is some learning or familiarity factor present here. Possibly the Snellen letter type of plates were more familiar to the subjects from previous experience in being tested with letter charts. It has been our experience that, generally speaking, observers are more willing to guess with letter targets in the region of the visual threshold than when using unfamiliar test targets such as checkerboards and circles.

In general, the reliability for near is lower than that for far; this is consistent with the findings of our earlier study.

The general magnitude of reliabilities is reasonably good on all but the Ortho-Rater near alley. This is undoubtedly due to the poor test target reproduction of Ortho-Rater targets for use in the alleys. As mentioned previously, each manufacturer was asked to handle reproduction of their test targets. Since the Submarine Base photographic facilities are greatly over-worked and limited in scope, we felt preparation of the necessary material by the companies according to specifications submitted to them would not only result in better material but also forestall the feeling by any or all companies that full justice had not been done to their test targets. As it turned out, the targets prepared by the manufacturers left much to be desired. The best photographic work was done by the American Optical Company, and the Telebinocular and New London Letters were mediocre, falling in between the American Optical and Bausch and Lomb reproductions. The Ortho-Rater test target reproduction was definitely the worst, and the consequent loss of detail involved is reflected in the low reliability for the Ortho-Rater near alley.

The same general tendency is true for validities as for reliabilities. They are generally lower than in our earlier study with the same exception noted for the reliabilities - that of the New London Letters is slightly higher. Once again, there is a poor showing by the Ortho-Rater near alley. The low validity for the Telebinocular near requires explanation: it may have been due to a slight defect in the oculars and ocular mount of this particular instrument; a new head for this device has since been supplied by the manufacturer.

There are no significant differences revealed in a comparison between test-retest reliabilities or between right eye, left eye or binocular.

Section II:

The test-retest scores were pooled, yielding one large sample of scores of 256 eyes to be further analyzed in order to determine whether more meaningful information could be obtained from these than from a single comparison of

validity coefficients. The variance of the pooled data equals one-half the sum of the test and retest variance plus one-fourth the square of the difference of the test and retest means. Tabular presentation is given in Tables V through VIII.

These means and variances are compared by the t and F test. The t test is a measure of the probability of getting by pure chance as great a difference or greater between means as those obtained in our sample, while the F test is a measure of the probability of getting by pure chance as great a dispersion or divergence of scores as those obtained in our sample.

We needed this mean-variance data on all instruments and alleys to clarify interpretation of the later tables where we have comparisons of various combinations of means and variances.

In Tables IX through XII we have considered RE/LE, RE/Bin and LE/Bin for each instrument near and far and for the visual alleys near and far. In addition, the comparison between right eye, left eye and binocular performance respectively was investigated between each instrument near versus visual alley near, and each instrument far versus visual alley far. Any items which demonstrated significant differences in means found are marked with asterisks. For example, note that Ortho-Rater instruments #1 and #2 compared to visual alley near (Table IX) yield significant differences on means.

To further consider whether any significant difference of means was due to a shift in the average score or to a real change in the dispersion of scores, the F test was applied to the variances, and significant ratios as determined by the F test are indicated by starred items in the F columns of these tables. The ratios in each case demonstrate which variable was considered the numerator and which the denominator.

Now we may state that the t and F tests have shown us that when there are significant differences on the t test, they are generally due to a shift in the mean score. Note that there is not much overlap in starred items in the "t" and "F" columns of Tables IX through XII.

The sizable shift in the means of the visual alley versus Ortho-Rater near alley is once more a reflection of inferior test target reproduction of slides for use in the alleys.

When in Table IX we examine the right eye performance between the instruments and their corresponding targets in the alleys we note that in all cases the significant difference of means is due to a genuine shift in the mean scores. Only in the case where the instrument is compared to the corresponding test targets in the near alley is the variance ratio significant. For the far tests with the Ortho-Rater there is only one case in which the variance ratio is significant.

Significant differences in means on the Sight Screener data (Table X) are similarly starred to facilitate interpretation. Again, in cases where the t test is significant and the F test is not, we assume that there has been a genuine shift in mean scores. We note that instrument far versus the corresponding targets in the alleys yield significant difference in means and significant ratios of variance.

Further, comparison of right eye and binocular mean scores is significant in all cases on the Sight Screener and New London Letter data whereas this comparison does not yield consistent significant differences for Ortho-Rater and Telebinocular data.

The Telebinocular results (Table XI) present a pattern similar to that of Sight Screener. Again we find the situation where the instrument near versus the corresponding test targets in the near alley yield significant differences in all means and in 3 out of 6 variance ratios: this holds for both the A and B scores.

Checking back to the intermediate tables V through VIII we find that the same trend was revealed there. However, the evidence was inconclusive until this further analysis substantiated the tendency.

Table XIII and XIV compare the means of the scores for each eye (right eye, left eye and binocular) for each instrument and for its corresponding alley targets with the New London Letters for near and far respectively. In the case of the near, we find that all yielded significant differences in mean scores with the sole exception of the Telebinocular visual alley B score. In the comparison of the far mean scores, we find that 14 out of 18 means yielded highly significant differences.

A hurried attempt was made to check the importance to be attached to the poor photographic reproduction of instrument targets by increasing the mean scores obtained for the New London Letters near chart by the addition of a suitable constant to raise the score level to conform to the expected mean score, and comparing the adjusted scores with mean scores for all near instrument and alley tests. In all cases the "t" value for instrument versus alley remained significant.

We conclude (tentatively in the absence of more data) that the specific instruments and their test targets when used in the alleys are measuring something different or in a different way than do the New London Letters.

Many variables doubtless affect visual scores and exert a consequent influence on the resultant "t" and "F" values. More definitive evidence for or against the relative significance reported requires more adequate control of such factors as:

1. Different test target illumination between the instruments and between instruments and alleys.

2. Different optical factors employed in the presentation of targets by the several screening instruments.

3. Different types of test charts used by the various devices.

4. Use of better photographic instrument target production for incorporation in alley charts.

All this suggests additional experimentation to determine the relative weight to be attached to such variables in order to provide guide lines for the improvement of existing instruments or the construction of a new one.

Discussion:

Dr. Fry asked whether refractive error data were available on the personnel used in the tests.

Commander Cook replied in the negative. He added that refractive corrections which the patients habitually wore were noted. Of the group of 128, there were not more than 25 who normally wore corrections.

Dr. Fry asked whether the patients were tested with refractive corrections.

Commander Cook replied that the patients did not wear their corrections during test even though they ordinarily wore them.

Dr. Hulburt asked whether the report could be considered a completed study of the commercial screening devices.

Commander Cook replied in the negative. He said that the work opened a series of new problems which needed to be explored.

Dr. Uhlaner asked whether the instrument tests had been validated against the New London letter chart.

Commander Cook replied in the affirmative.

Dr. Uhlaner asked what would have happened if the comparison had been made against some of the other letter charts such as the special checkerboard charts used by the AGO study.

Commander Cook expressed his belief that much the same results would have been obtained had the comparisons been made against other test objects.

TABLE I

Means, Standard Deviations, Reliability Coefficients (r) and Validity Coefficients* (r_v)

for Test and Retest ORTHORATER Scores

N = 128

		Test		Retest			Test			Retest		
		Mean	S.D.	Mean	S.D.	r	r_v-RE	r_v-LE	r_v-BIN	r_v-RE	r_v-LE	r_v-BIN
Instrument #1, Near	RE	10.76	2.50	11.17	2.41	.557	.599			.669		
	LE	11.40	2.45	11.76	2.36	.618		.643			.687	
	BIN	11.37	2.10	11.28	1.89	.599			.548			.660
Instrument #2, Near	RE	10.30	2.09	10.80	2.07	.618	.599			.602		
	LE	11.48	2.26	11.70	2.24	.644		.652			.616	
	BIN	10.88	2.17	11.17	2.25	.537			.672			.671
Instrument #1, Far	RE	10.13	2.93	10.48	2.79	.667	.652			.696		
	LE	10.12	2.54	10.66	2.85	.635		.644			.698	
	BIN	11.09	2.58	11.24	2.60	.686			.627			.641
Instrument #2, Far	RE	10.38	2.50	10.20	2.51	.692	.704			.709		
	LE	10.55	2.43	10.73	2.59	.697		.688			.695	
	BIN	10.93	2.36	10.98	2.32	.667			.731			.639
Visual Alley, Near	RE	7.85	2.30	7.70	2.24	.294	.389			.333		
	LE	8.01	2.14	8.34	2.18	.306		.531			.462	
	BIN	8.57	2.35	8.54	2.38	.305			.388			.448
Visual Alley, Far	RE	9.38	2.80	9.66	2.66	.608	.675			.637		
	LE	9.84	2.81	10.17	2.86	.540		.598			.603	
	BIN	10.84	2.91	10.43	2.92	.577			.598			.730

Means decimal equivalents of Snellen Fractions; Means and Standard Deviations have been multiplied by a factor of 10.

* Validity Coefficients (r_v) are computed using the mean of the RE, LE and BIN scores on the New London Letters for Near and Far tests respectively.

TABLE II

Means, Standard Deviations, Reliability Coefficients (r) and Validity Coefficients* (r_v) for Test and Retest SIGHT SCREENER Scores

N = 128

		Test		Retest			Test			Retest		
		Mean	S.D.	Mean	S.D.	r	r_v-RE	r_v-LE	r_v-BIN	r_v-RE	r_v-LE	r_v-BIN
Instrument, Near	RE	9.76	3.40	10.20	3.59	.642	.757			.667		
	LE	10.65	3.44	10.94	3.60	.576		.698			.638	
	BIN	10.99	3.20	11.45	3.72	.579			.727			.695
Instrument, Far	RE	9.44	3.36	9.88	3.63	.664	.694			.696		
	LE	10.02	3.41	10.59	3.87	.568		.673			.644	
	BIN	11.41	3.74	11.72	4.11	.596			.626			.676
Visual Alley, Near	RE	10.44	3.63	10.69	3.73	.683	.770			.717		
	LE	10.52	3.52	11.16	3.70	.652		.770			.696	
	BIN	11.62	3.26	12.87	4.04	.614			.703			.704
Visual Alley, Far	RE	11.66	4.10	12.30	4.50	.706	.769			.764		
	LE	11.88	4.31	12.55	4.68	.686		.744			.771	
	BIN	14.27	4.79	14.78	4.96	.703			.778			.763

Means are decimal equivalents of Snellen Fractions; Means and Standard Deviations have been multiplied by a factor of 10.

* Validity Coefficients (r_v) are computed using the mean of the RE, LE and BIN scores on the New London Letters for Near and Far tests respectively.

TABLE III

Means, Standard Deviations, Reliability Coefficients (r) and Validity Coefficients* (r_v) for Test and Retest TELEBINOCULAR Scores

N = 128

A Score = Lab. Score
B Score = Co. Score

		Test		Retest			Test			Retest		
		Mean	S.D.	Mean	S.D.	r	r_v-RE	r_v-LE	r_v-BIN	r_v-RE	r_v-LE	r_v-BIN
Instrument, Near-A Score	RE	15.79	3.18	16.56	3.42	.551	.356			.430		
	LE	16.77	4.23	17.73	3.93	.502		.460			.443	
	BIN	16.88	3.46	17.38	3.45	.499			.513			.485
B Score	RE	8.53	2.01	8.98	2.01	.675	.443			.459		
	LE	8.63	2.34	9.41	2.09	.524		.549			.471	
	BIN	9.07	1.97	9.52	2.11	.483			.511			.410
Visual Alley, Near-A Score	RE	11.26	3.02	11.91	2.85	.492	.567			.491		
	LE	11.89	3.63	12.25	3.76	.527		.629			.597	
	BIN	11.74	2.70	12.13	2.70	.509			.579			.489
B Score	RE	6.82	1.91	7.12	1.82	.620	.696			.618		
	LE	5.96	1.76	6.16	1.74	.502		.581			.660	
	BIN	6.45	1.50	6.79	1.68	.675			.656			.561
Visual Alley, Far-A Score	RE	13.38	3.74	14.16	3.42	.458	.513			.617		
	LE	15.35	5.36	15.91	4.54	.498		.587			.521	
	BIN	15.23	4.85	15.75	4.17	.466			.564			.556
B Score	RE	6.84	2.04	7.44	1.84	.528	.521			.641		
	LE	6.94	2.28	7.54	1.90	.504		.548			.590	
	BIN	6.13	1.61	6.40	1.55	.536			.411			.402

Means are decimal equivalents of Snellen Fractions; Means and Standard Deviations have been multiplied by a factor of 10.

* Validity Coefficients (r_v) are computed using the mean of the RE, LE and BIN scores on the New London Letters for Near and Far tests respectively.

Linear relations between A and B near scores are approximately expressed by $(\text{A Score})_{RE} = -7.3 + 2.7\,(\text{B Score})_{RE}$; $(\text{A Score})_{LE} = 1.2 + 1.8\,(\text{B Score})_{LE}$; $(\text{A Score})_{BIN} = -.9 + 1.9\,(\text{B Score})_{BIN}$

TABLE IV

Means, Standard Deviations, Reliability Coefficients (r) and Validity Coefficients* (r_v)

for Test and Retest NEW LONDON LETTERS Scores

N = 128

		Test		Retest				Test			Retest	
		Mean	S.D.	Mean	S.D.	r	r_v-RE	r_v-LE	r_v-BIN	r_v-RE	r_v-LE	r_v-BIN
Visual Alley, Near	RE	6.27	2.29	6.52	2.45	.742	.865			.862		
	LE	6.39	2.34	6.66	2.40	.748		.875			.862	
	BIN	7.97	2.49	7.90	2.43	.801			.873			.898
Visual Alley, Far	RE	7.86	3.00	8.02	3.23	.818	.897			.859		
	LE	8.36	3.17	8.51	3.58	.754		.854			.893	
	BIN	10.31	3.62	10.49	3.63	.853			.925			.925

Means are decimal equivalents of Snellen Fractions; Means and Standard Deviations have been multiplied by a factor of 10.

* Validity Coefficients (r_v) are computed using the mean of the RE, LE and BIN scores on the New London Letters for Near and Far tests respectively.

TABLE V

Means and Variances* for Pooled Test-Retest ORTHORATER Scores

N = 256

		Mean	Variance
Instrument #1, Near	RE	10.96	6.07
	LE	11.58	5.83
	BIN	11.32	3.98
Instrument #2, Near	RE	10.55	4.38
	LE	11.59	5.07
	BIN	11.02	4.90
Instrument #1, Far	RE	10.30	8.22
	LE	10.39	7.38
	BIN	11.16	6.73
Instrument #2, Far	RE	10.29	6.28
	LE	10.64	6.32
	BIN	10.96	5.49
Visual Alley, Near	RE	7.77	5.16
	LE	8.18	4.69
	BIN	8.55	5.58
Visual Alley, Far	RE	9.52	7.47
	LE	10.00	8.06
	BIN	10.63	8.52

* Variance of pooled data equals one-half the sum of the Test and Retest variances plus one-quarter the square of the differences of the Test and Retest means.

TABLE VI

Means and Variances* for Pooled Test-Retest SIGHT SCREENER Scores

N = 256

		Mean	Variance
Instrument, Near	RE	9.98	12.28
	LE	10.80	12.41
	BIN	11.22	12.10
Instrument, Far	RE	9.66	12.26
	LE	10.30	13.40
	BIN	10.57	15.48
Visual Alley, Near	RE	10.56	13.55
	LE	10.84	13.14
	BIN	12.25	13.85
Visual Alley, Far	RE	11.98	18.63
	LE	12.22	20.37
	BIN	14.53	23.87

* Variance of pooled data equals one-half the sum of the Test and Retest variances plus one-quarter of the Differences of the Test and Retest means.

TABLE VII

Means and Variances* for Pooled Test-Retest TELEBINOCULAR Scores

N = 256

			Mean	Variance
Instrument, Near	A Score	RE	16.18	11.06
		LE	17.25	16.90
		BIN	17.13	11.97
	B Score	RE	8.76	4.08
		LE	9.02	5.08
		BIN	9.29	4.22
Visual Alley, Near	A Score	RE	11.59	8.72
		LE	12.07	13.66
		BIN	11.94	7.32
	B Score	RE	6.97	3.50
		LE	6.06	3.07
		BIN	6.62	2.57
Visual Alley, Far	A Score	RE	13.77	12.96
		LE	15.63	24.73
		BIN	15.49	20.54
	B Score	RE	7.14	3.88
		LE	7.24	4.49
		BIN	6.27	2.52

* Variance of Pooled data equals one-half the sum of the Test and Retest variances plus one-quarter of the square of the differences of the Test and Retest means.

Linear relations between A and B near scores are approximately expressed by $(\text{A Score})_{RE} = -7.3 + 2.7\,(\text{B Score})_{RE}$; $(\text{A Score})_{LE} = 1.2 + 1.8\,(\text{B Score})_{LE}$; $(\text{A Score})_{BIN} = -.9 + 1.9\,(\text{B Score})_{BIN}$.

TABLE VIII

Means and Variances* for Pooled Test-Retest NEW LONDON LETTERS Scores

N = 256

		Mean	Variance
Visual Alley, Near	RE	6.39	5.61
	LE	6.53	5.62
	BIN	7.93	6.04
Visual Alley, Far	RE	7.94	9.72
	LE	8.43	11.44
	BIN	10.40	13.13

* Variance of pooled data equals one-half the sum of the Test and Retest variances plus one-quarter of the square of the differences of the Test and Retest means.

TABLE IX

"t" and "F" values for pooled (Test and Retest) ORTHORATER Scores

	Comparison between	t	Comparison between	F
Instrument #1, Near	RE - LE	2.84**	LE/RE	.96
	RE - BIN	1.80	BIN/RE	.66**
	LE - BIN	1.30	BIN/LE	.68*
Instrument #2, Near	RE - LE	5.40**	LE/RE	1.16
	RE - BIN	2.48*	BIN/RE	1.12
	LE - BIN	2.86**	BIN/LE	.97
Instrument #1, Far	RE - LE	.32	LE/RE	.90
	RE - BIN	3.54**	BIN/RE	.82
	LE - BIN	3.30**	BIN/LE	.91
Instrument #2, Far	RE - LE	1.58	LE/RE	1.00
	RE - BIN	3.10**	BIN/RE	.87
	LE - BIN	1.48	BIN/LE	.87
Visual Alley, Near	RE - LE	2.04*	LE/RE	.91
	RE - BIN	3.80**	BIN/RE	1.08
	LE - BIN	1.88	BIN/LE	1.19
Visual Alley, Far	RE - LE	1.98*	LE/RE	1.08
	RE - BIN	4.46**	BIN/RE	1.14
	LE - BIN	2.46*	BIN/LE	1.06
Instrument #1, Near vs. Visual Alley, Near	RE - RE	15.20**	Instr./Alley	1.18
	LE - LE	16.80**	"	1.24
	BIN - BIN	14.40**	"	.71*
Instrument #2, Near vs. Visual Alley, Near	RE - RE	14.40**	"	.85
	LE - LE	17.40**	"	1.08
	BIN - BIN	12.20**	"	.88
Instrument #1, Near vs. Instrument #2, Near	RE - RE	2.04*	In.#1/In.#2	1.39*
	LE - LE	.06	"	1.15
	BIN - BIN	1.60	"	.81
Instrument #1, Far vs. Instrument #2, Far	RE - RE	.06	"	1.31
	LE - LE	1.10	"	1.17
	BIN - BIN	.94	"	1.23
Instrument #1, Far vs. Visual Alley, Far	RE - RE	3.18**	Instr./Alley	1.10
	LE - LE	1.56	"	.92
	BIN - BIN	2.18*	"	.79
Instrument #2, Far vs. Visual Alley, Far	RE - RE	3.34**	"	.84
	LE - LE	2.68**	"	.75
	BIN - BIN	1.38	"	.64**

* Significant on 5% level for "t" and "F" values
** Significant on 1% level for "t" and "F" values

TABLE X

"t" and "F" values for pooled (Test and Retest) SIGHT SCREENER Scores

	Comparison between	t	Comparison between	F
Instrument, Near	RE - LE	2.62**	LE/RE	1.01
	RE - BIN	4.00**	BIN/RE	.98
	LE - BIN	1.36	BIN/LE	.97
Instrument, Far	RE - LE	2.04*	LE/RE	1.09
	RE - BIN	5.78**	BIN/RE	1.26
	LE - BIN	3.76**	BIN/LE	1.16
Visual Alley, Near	RE - LE	.84	LE/RE	.97
	RE - BIN	5.14**	BIN/RE	1.02
	LE - BIN	4.34**	BIN/LE	1.05
Visual Alley, Far	RE - LE	.60	LE/RE	1.09
	RE - BIN	6.24**	BIN/RE	1.28
	LE - BIN	5.56**	BIN/LE	1.17
Instrument, Near vs. Alley, Near	RE - RE	1.84	Instr./Alley	.91
	LE - LE	.12	"	.94
	BIN - BIN	3.22**	"	.87
Instrument, Far vs. Alley, Far	RE - RE	6.68**	"	.66**
	LE - LE	5.26**	"	.66**
	BIN - BIN	7.54**	"	.65**

* Significant on 5% level for "t" and "F" values
** Significant on 1% level for "t" and "F" values

TABLE XI

"t" and "F" values for pooled (Test and Retest) TELEBINOCULAR Scores

	Comparison between	t	Comparison between	F
Instrument, Near - A Score	RE - LE	3.24**	LE/RE	1.53**
	RE - BIN	3.18**	BIN/RE	1.08
	LE - BIN	.34	BIN/LE	.71*
Instrument, Near - B Score	RE - LE	1.40	LE/RE	1.24
	RE - BIN	2.96**	BIN/RE	1.03
	LE - BIN	1.40	BIN/LE	.83
Visual Alley, Near - A Score	RE - LE	1.64	LE/RE	1.57**
	RE - BIN	1.40	BIN/RE	.84
	LE - BIN	.46	BIN/LE	.54**
Visual Alley, Near - B Score	RE - LE	5.70**	LE/RE	.88
	RE - BIN	2.28*	BIN/RE	.73*
	LE - BIN	3.80**	BIN/LE	.84
Visual Alley, Far - A Score	RE - LE	4.84**	LE/RE	1.91**
	RE - BIN	4.74**	BIN/RE	1.58**
	LE - BIN	.34	BIN/LE	.83
Visual Alley, Far - B Score	RE - LE	.52	LE/RE	1.16
	RE - BIN	5.54**	BIN/RE	.65**
	LE - BIN	5.86**	BIN/LE	.56**
Instrument, Near vs. Alley, Near - A Score	RE - RE	16.48**	Instr./Alley	1.27
	LE - LE	14.96**	"	1.24
	BIN - BIN	18.88**	"	1.63**
Instrument, Near vs. Alley, Near - B Score	RE - RE	10.36**	"	1.17
	LE - LE	16.62**	"	1.65**
	BIN - BIN	16.40**	"	1.64**

* Significant on 5% level for "t" and "F" values
** Significant on 1% level for "t" and "F" values

Linear relations between A and B near scores are approximately expressed by $(A\ Score)_{RE} = -7.3 \neq 2.7\ (B\ Score)_{RE}$; $(A\ Score)_{LE} = 1.2 \neq 1.8\ (B\ Score)_{LE}$; $(A\ Score)_{BIN} = -.9 \neq 1.9\ (B\ Score)_{BIN}$

TABLE XII

"t" and "F" values for pooled (Test and Retest) NEW LONDON LETTERS Scores

	Comparison between	t	Comparison between	F
Visual Alley, Near	RE - LE	.66	LE/RE	1.00
	RE - BIN	7.22**	BIN/RE	1.08
	LE - BIN	6.58**	BIN/LE	1.08
Visual Alley, Far	RE - LE	1.72	LE/RE	1.18
	RE - BIN	8.22**	BIN/RE	1.35*
	LE - BIN	6.34**	BIN/LE	1.15

* Significant on 5% level for "t" and "F" values
** Significant on 1% level for "t" and "F" values

TABLE XIII

Comparison of Means of NEW LONDON LETTERS Near, Test, Scores
with Means of all other Near, Test, Scores

N = 128

		Mean	S.D.			Mean	S.D.	t
ORTHORATER	RE	10.76	2.50	NEW LONDON	RE	6.27	2.29	14.92**
Instrument #1	LE	11.40	2.45	LETTERS	LE	6.39	2.34	16.64**
	BIN	11.37	2.10		BIN	7.97	2.49	11.76**
ORTHORATER	RE	10.30	2.09	"	RE	6.27	2.29	14.65**
Instrument #2	LE	11.48	2.26		LE	6.39	2.34	17.61**
	BIN	10.88	2.17		BIN	7.97	2.49	9.93**
ORTHORATER	RE	7.85	2.30	"	RE	6.27	2.29	5.49**
Visual Alley	LE	8.01	2.14		LE	6.39	2.34	5.77**
	BIN	8.57	2.35		BIN	7.97	2.49	2.00*
SIGHT SCREENER	RE	9.76	3.40	"	RE	6.27	2.29	9.59**
Instrument	LE	10.65	3.44		LE	6.39	2.34	11.54**
	BIN	10.99	3.20		BIN	7.97	2.49	8.39**
SIGHT SCREENER	RE	10.44	3.63	"	RE	6.27	2.29	10.94**
Visual Alley	LE	10.52	3.52		LE	6.39	2.34	11.01**
	BIN	11.62	3.26		BIN	7.97	2.49	10.03**
TELEBINOCULAR	RE	8.53	2.01	"	RE	6.27	2.29	8.37**
Instrument	LE	8.63	2.34		LE	6.39	2.34	7.65**
B Score	BIN	9.07	1.97		BIN	7.97	2.49	3.90**
TELEBINOCULAR	RE	6.82	1.91	"	RE	6.27	2.29	2.08*
Visual Alley	LE	5.96	1.76		LE	6.39	2.34	1.65
B Score	BIN	6.45	1.50		BIN	7.97	2.49	5.89**

* Significant on 5% level
** Significant on 1% level

TABLE XIV

Comparison of Means of NEW LONDON LETTERS Far, Test, Scores
with Means of all other Far, Test, Scores

N = 128

		Mean	S.D.			Mean	S.D.	t
ORTHORATER	RE	10.13	2.93	NEW LONDON	RE	7.86	3.00	6.10**
Instrument #1	LE	10.12	2.54	LETTERS	LE	8.36	3.17	4.89**
	BIN	11.09	2.58		BIN	10.31	3.62	1.98
ORTHORATER	RE	10.38	2.50	"	RE	7.86	3.00	7.26**
Instrument #2	LE	10.55	2.43		LE	8.36	3.17	6.19**
	BIN	10.93	2.36		BIN	10.31	3.62	1.62
ORTHORATER	RE	9.38	2.80	"	RE	7.86	3.00	4.18**
Visual Alley	LE	9.84	2.81		LE	8.36	3.17	3.94**
	BIN	10.84	2.91		BIN	10.31	3.62	1.29
SIGHT SCREENER	RE	9.44	3.36	"	RE	7.86	3.00	3.95**
Instrument	LE	10.02	3.41		LE	8.36	3.17	4.02**
	BIN	11.41	3.74		BIN	10.31	3.62	2.38*
SIGHT SCREENER	RE	11.66	4.10	"	RE	7.86	3.00	8.42**
Visual Alley	LE	11.88	4.31		LE	8.36	3.17	7.41**
	BIN	14.27	4.79		BIN	10.31	3.62	7.43**
TELEBINOCULAR	RE	6.84	2.04	"	RE	7.86	3.00	3.17**
Visual Alley	LE	6.94	2.28		LE	8.36	3.17	4.09**
B Score	BIN	6.13	1.61		BIN	10.31	3.62	11.91**

* Significant on 5% level
** Significant on 1% level

AN EXPERIMENTAL VALIDATION OF CHECKERBOARD TARGETS FOR MEASURING VISUAL ACUITY

BY

Leon M. Rudolph

The Sub-Committee on Vision Testing has shown the checkerboard type of visual acuity target to be one of the most reliable for determing visual resolution. In some of the preliminaries of our night vision experiments it was noticed that observers could utilize cues other than visual resolution as a basis for their responses. This appears to be due to the fact that, although the targets were produced photographically with considerable care, differences occur in the reflectivity of the black elements which show up as brightness differences in various parts of the target. Since the test is based on the assumption that at certain distances (or brightnesses) resolution of the checkerboard section becomes equivocal, the fusion of its elements should give a grey that is indistinguishable from the fusion of the dots in the other three grids. Any factor that can act as a cue to differentiate the four squares destroys the utility of that target.

Since these targets are to be used in the scotopic level of illumination, as well as the photopic, it is especially important to eliminate brightness differences, because brightness discrimination increases in low illumination, i.e., below 3 F.C.

The purpose of our study was to eliminate those targets giving these secondary cues. (Not included in our study were those showing obvious gross imperfections, overprinting, etc.; they were rejected immediately.)

The literature does not show any attempt to validate individual visual acuity targets. In the first experimental use of the checkerboard target, Giese admitted that brightness differences between the checkerboard and the stippled patterns, if any, were not tested!

Using the AGO formula for a log series of visual acuity steps in which $V = \log_2 \frac{2^{10}}{A \text{ (visual angle)}}$, we have prepared our targets in sizes shown in table #1. From this table, we can easily see the relationship between each target and visual angle, etc.

In the procedure, a target was presented twenty times with the checkerboard in the four positions - upper right, lower left, upper left and lower right - in a predetermined random order. The brightness of the target was of the order of 7 foot lamberts. Testing conditions were kept as close as possible to those set down in the recommendations of the Sub-Committee on Visual Testing. Observations were made binocularly and the observer was not allowed to give a "no" response. A set of observations was obtained at 5 distances from the testing target, ranging from 1/4 log unit position before that at which 1' of visual angle is subtended, to three 1/4 log units beyond that point (e.g. for the 20' target, from 17' to 34'). It was at these points that we expected, from preliminary observations, approximately 100% and 25% correct responses, respectively.

Four seconds were allowed for the observer to give his response.

Data were obtained on 102 targets using 3 observers per target. Many observers served for more than one target. Every prospective observer was given an optometric screening, and was not used if showing any of the following disqualifications: media or fundus changes, general fatigue, less than 20/20 vision and gross hyperopic errors. Observers were obtained from enlisted personnel of the Submarine Base and ranged in ages from 18 to 27. The range of visual acuity was from 20/20 to 20/15.

We know that when a large number of components go to make up a visual perception, a sigmoid curve may be expected. Each one of these components functions less well as the task becomes more difficult. The general characteristic of such a curve would be its flatness at the ends and its steepness in the middle. Thus, a quite large change in size of target would have little effect in slope of the curve, where the task is easy, or again, where it is difficult. At intermediate points, however, slight changes in size produce a relatively great change in number of correct responses.

To get a standard curve to which we can compare the individual curves for purposes of rejection or acceptance, we first plotted all the curves of the individual targets within a size group with their average curve. (2) It must be remembered that targets having curves falling much to one side tend to draw the average to that side. Consequently, were we to use the average, after limits have been set for rejection purposes, targets with satisfactory curves might be erroneously rejected. Accordingly, the medians were used for comparison purposes. To determine the actual acceptance of individual targets, there is indicated a need for setting boundaries or limits, within which each target curve must fall. Since we are using decrements of 1/4 log steps, the targets we finally accept should fall along the median line slope, within a 1/4 log boundary, i.e., 1/8 log unit to each side of the median. Using this basis, then, we have an area bounded by continuous curves, to each side of the median. It should be expressly stated that these limits were set in accordance with our experimental needs only, and that no attempt has been made to evaluate the checkerboard target in general as a measure of visual acuity. No measurable limits were set for rejection purposes. Each curve (target) falling out of our limits was subject to rejection after consideration of its individual merits.

In an interpretation of the different curves leading to rejection which have and might have occurred, we have the following considerations:

If the curve reaches the threshold or up to 100% correct responses more quickly than the median, outside our limits, that would indicate the observers may have used a secondary cue, such as darkness or "difference" of the checkerboard, in giving their responses. (3-curve A)

If the brightnesses of more than two of the four squares differed, the confusion of the observer as to which position to call would show itself in a long drawn-out curve, reaching the threshold or above it, at a closer distance to the target than does the median with its boundaries. 3 (curve B)

If the curve does not follow the general slope of the median curve, but fluctuates wildly inside and outside the limits, it might indicate the observers were picking up a combination of secondary cues, first using one, then the other.

The checkerboard may appear the darkest of the squares, even at three 1/4 log steps away from the 1' visual angle position, and the observer using that cue would never reach very near the 25% correct responses level. (3-curve C)

At the lower end of the sigmoid curve, if we get a plateau of all or practically all incorrect responses, the observers probably were using the "different" brightnesses of one of the three grey squares as their cue, and called most responses, then, incorrectly. (3-curve d)

If we get a curve that follows the general slope of the median to a distance beyond the log 10.00 position, then sharply increases in number of correct responses, there is indicated that the observers have changed from one method of determing his response to another. At the further distances he may have used a secondary cue such as the brightness of the checkerboard, after failing to resolve the actual squares. As he moved to a position closer to the target, he again attempted to respond by resolving the squares. (3-curve e)

It may be noted that the judgment of the performance by the observer was of no value, as far as correct or incorrect responses were concerned; for instance, when the observers thought they were getting all responses incorrect, they actually were getting near 25% correct.

If we wish, now, to determine the most sensitive part of the curve, and that point at which we can actually "see" the target - our limen - we may first provide a clearer picture of the spread of the correct responses at the different distances. Histograms, therefore, were prepared, and, as we note in the 20' target histogram, (4) the greatest spread lies at the point where 1' of visual angle is subtended (log. 10.00).

It has been shown that to get the area of the threshold, the best part of the curve to deal with would be that part near the center, where a given change in size produces the maximal effect on the number of correct responses.

To get an over-all picture of these maximal effects, we have plotted the mean variation for each group of targets in terms of log size of the target. We note that the greatest MV occurs at log 10.00 (1' of visual angle). (5) It might seem that, were we to plot the MV of the groups from their average, at the various log distances, the point of inflexion would vary. This was done, and the curve in Fig. 6 resulted. The dip is also greatest at log 10.00, indicating that we might accept that point as the one at which the threshold may be taken.

Aside from the information we have already drawn from our data, we may make the following secondary observations:

At log 10.00 - at which point we considered the threshold to lie - the average number of correct responses fell at 66.74%. 1/4 log unit before gave 92%, and 1/8 log unit more would have given us 100% correct. 1/2 log unit beyond log 10.00 gave us 26.53% correct responses, and three 1/4 log units beyond gave us 24.39%. On comparing the average number of correct responses at log 10.00 for the first smaller targets tested (7' to 14' targets), we notice their average correct responses is 73.5%, for the larger four (24' - 70) we have an average of 59.75%. The average of the two groups lies at 66.62%. It is interesting to note that the average for the 20 ft. target is 66.84%. All this, then, might indicate a decrease in visual acuity above the 20' testing distance, and an increase below it. And, as may be noted, larger targets at farther distances

gave us lower acuities than smaller targets at nearer distances all subtending the same visual angle.

In summary, then:

It was believed that secondary cues, such as brightness differences, may exist between targets of the same size, which would be misleading to observers. Our photographically reproduced visual acuity targets, therefore, were experimentally validated. 306 validations were made, taking over 30,600 observations, testing 9 differently sized targets, 102 targets in all. Of these 15% were rejected. A photoelectric measuring device is being set up and an attempt will be made to quantitatively measure brightness differences between the rejected and accepted targets. It is believed that we have shown secondary cues to exist in some targets of the checkerboard type and believe other visual acuity targets may present similar faults, on photographic reproduction. Accordingly, it is hoped that work will be done on validation of visual acuity targets in order to eliminate those with secondary cues before accepting them for experimental or clinical use.

Discussion

Dr. Uhlaner asked whether an initial selection had been made of the checkerboard targets used, to eliminate those with obvious brightness differences. The AGO study, for example, eliminated all such obviously aberrant charts.

Dr. Rudolph replied that they had screened all obviously irregular prints before the test had begun. He described a photoelectric device which is being delivered to provide a more precise means of selecting charts with equivalent brightness values in the four squares.

Dr. Blackwell asked about one of the results Dr. Rudolph reported, concerning the effect of distance upon acuity. He asked Dr. Rudolph the range of distances tested.

Dr. Rudolph replied that testing distances ranged from 7 to 34 feet.

Dr. Blackwell then asked whether, over this range of distances, there had been a difference in visual acuity at the various distances.

Dr. Rudolph replied that this was true and that they discovered better acuity at the shorter distances than at the longer distances.

In reply to a question from Dr. Blackwell, Dr. Rudolph replied that this was true for the satisfactory targets as well as for the rejects.

Dr. Blackwell then asked whether the angular size of the target surround was maintained angularly constant or physically constant when viewed at the various distances.

Dr. Rudolph replied that the physical size of the chart on which the test objects appeared was maintained constant, which resulted in an increasingly smaller uniform surround at the longer distances.

Dr. Blackwell indicated the possibility that the decrease in acuity at the longer distances resulted from the decrease in the angular size of the surround of the target objects, as indicated by the results of Fry & Bartley.

Dr. Blackwell reported that recent experiments conducted at Michigan indicated no variation in acuity as a function of distance from 1.5 to 20 feet at high brightness levels, provided the angular size of the surround was maintained constant. He reported, in addition, that at zero brightness, acuity was significantly worse at the shortest distances, the reverse finding to that of Dr. Rudolph.

Dr. Dimmick remarked that experiments are now in progress in which the entire visual field is relatively uniform at the various distances.

Dr. Blackwell predicted that under these circumstances the distance effect found by Dr. Rudolph would not appear.

Dr. Sloan asked whether there was reason to believe that different slides of the transilluminated type varied among themselves as much as the photographically prepared reflection slides.

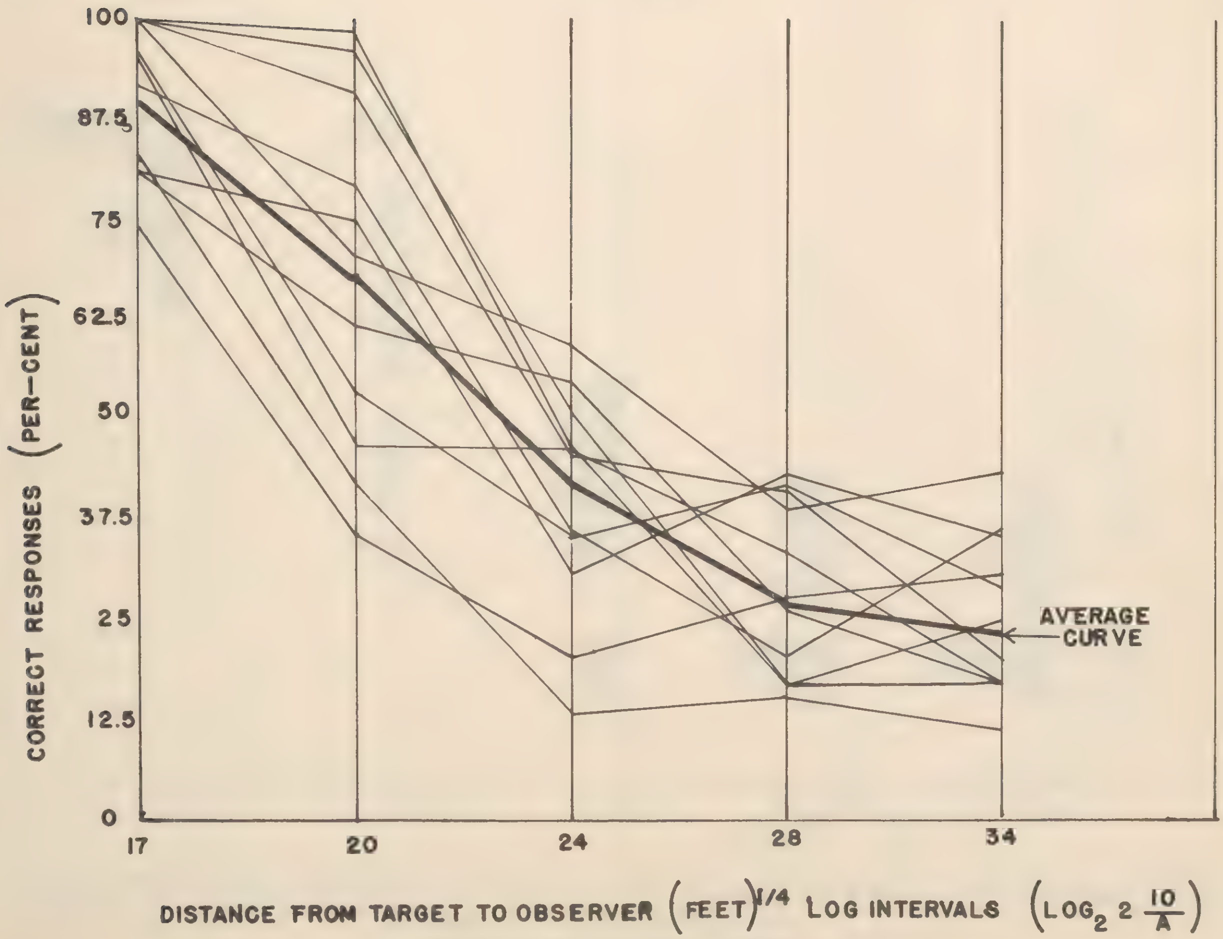
Commander Cook stated his belief that fairly good inspections of transilluminated slides was maintained by the manufacturers.

FIG. 1

$\log 2\frac{2^{10}}{A}$	Feet at which 1' visual angle is subtended	Min. of Vis. angle	Width of unit in inches
11.50	7.08	.354	.02470
11.25	8.42	.421	.02933
11.00	10.00	.500	.03492
10.75	11.90	.595	.04156
10.50	14.16	.708	.04940
10.25	16.84	.842	.05866
10.00	20.00	1.000	.06984
9.75	23.78	1.189	.08311
9.50	28.12	1.416	.09880
9.25	33.68	1.684	.11732
9.00	40.00	2.000	.13968
8.75	47.56	2.378	.16622
8.50	56.64	2.832	.19760
8.25	67.40	3.370	.23500
8.00	80.00	4.000	.27900

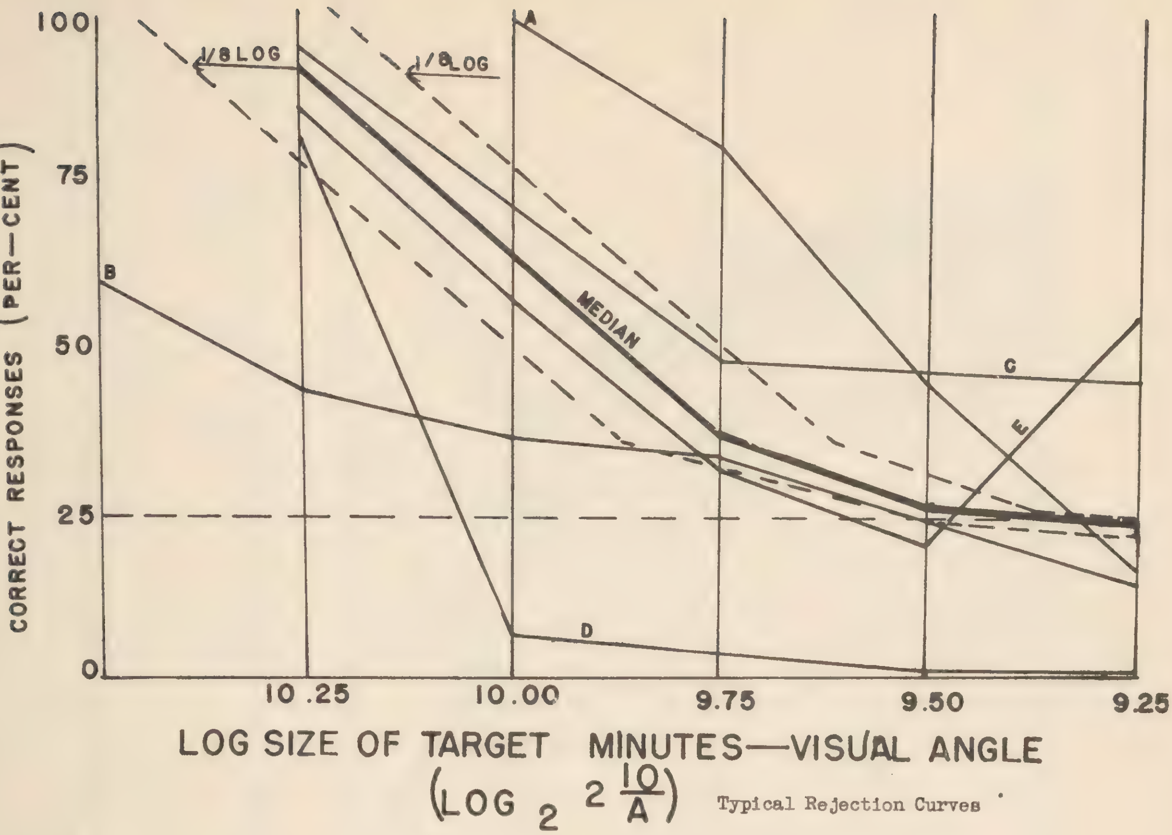
Table showing basis for construction of checkerboard targets.

FIG.2



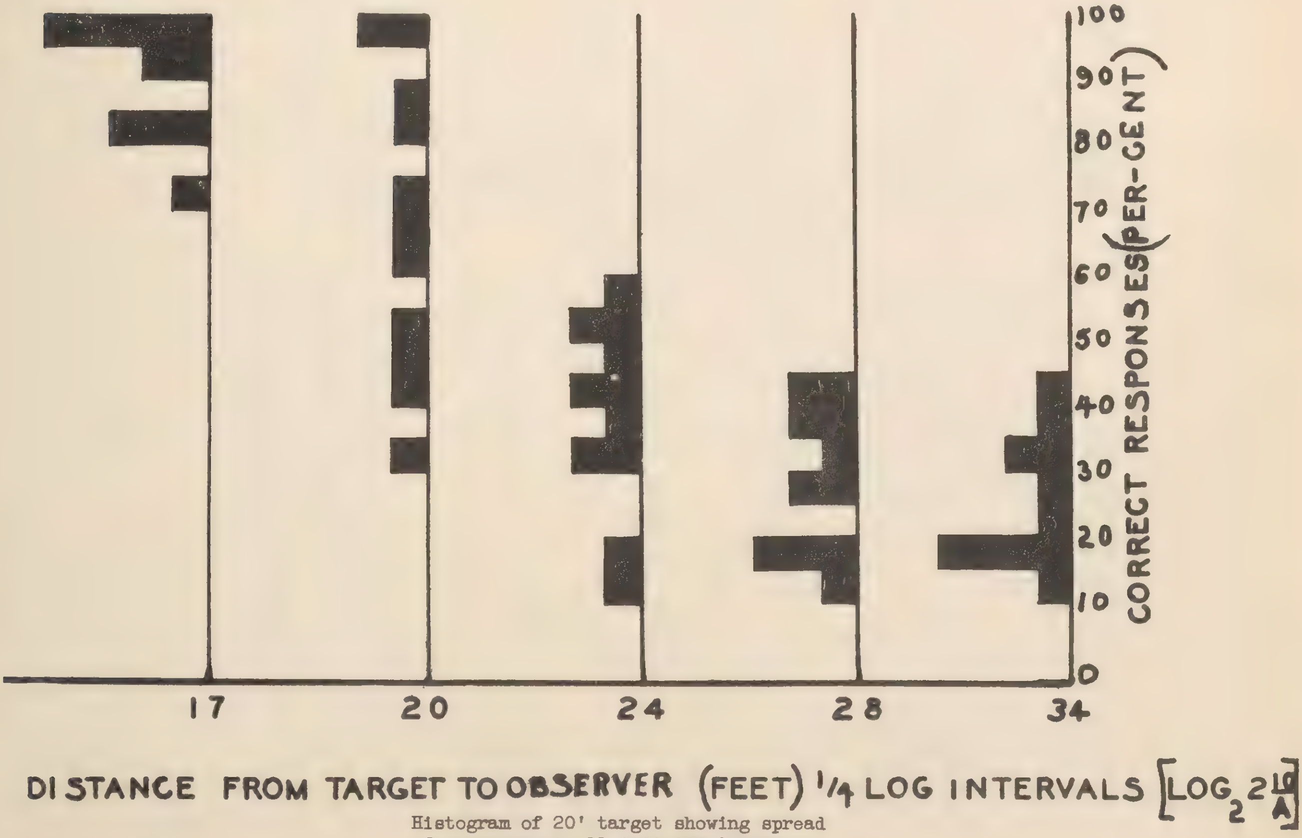
Sigmoid curves for eleven targets subtending one minute at 20 feet.

FIG.3



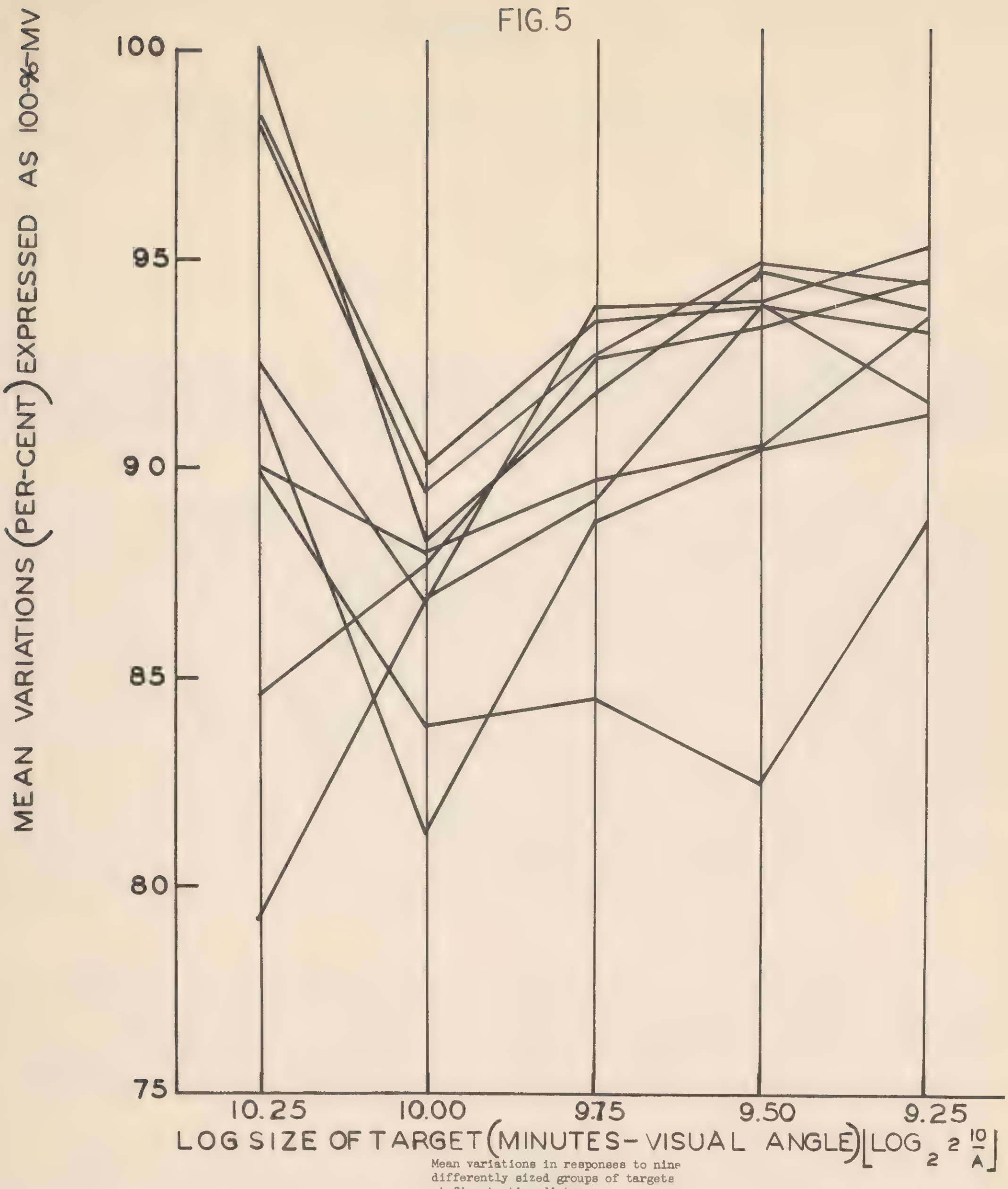
Typical Rejection Curves

FIG.4



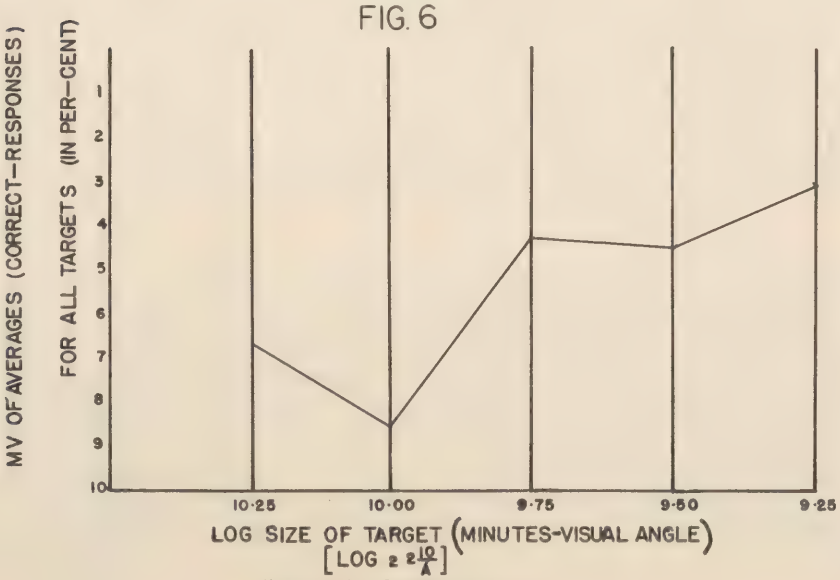
Histogram of 20' target showing spread of responses to 11 individual targets within 20' group.

FIG. 5



Mean variations in responses to nine differently sized groups of targets at five testing distances.

FIG. 6



Mean variation of average correct responses for nine differently-sized group of targets at 5 testing distances.

A STUDY OF DARK ADAPTATION WITH PARTICULAR REFERENCE TO METHOD FOR TESTING NIGHT VISION

BY

Forrest L. Dimmick

The title of the present paper is quite indefinite. What I intend to do is to describe three factors in night vision about which there is little information, but which determine the course of a study of night vision and the development of tests of scotopic sensitivity.

The first item which I propose to discuss is a methodological critique that has resulted from our exploration of Livingston's proposal that the map of the foveal blind spot in twilight vision be used as a measure of sensitivity to low illumination. His procedure consisted of moving a small weak stimulus (area 2 mm, brightness (3 to 9) x 10^{-9}C.P.) outward from a red fixation point until a subject reported that he saw the stimulus. The points on various radii at which the stimulus was first seen were taken as defining the limits of the central scotoma. This technique is typical of scotometric measurements. Its crudity is obvious and comments have been made, for example, about the rate of movement of the stimulus. One device has been worked out to control this factor somewhat, but no constructive changes in method have been suggested. We attempted, therefore, to set up a much more rigid technique of experimental method on which to base a better testing procedure, because we found that the test-retest reliability of Livingston's method was very poor.

We proposed to map the scotomata of several trained observers (6) with a number of repeated determinations for each observer. We planned to discover the influence of the stimulus variables: size, intensity, and rate and direction of movement. The last variable, direction of movement, proved so important a factor in determining the boundaries of the scotoma that we devoted all of our time to it.

Our data show that two entirely divergent determinations of the extent of the blind spot are obtained when the stimulus is moved in the two directions, (1) from the blind area out to first visibility as Livingston prescribed, and (2) in from visibility in the periphery to the point of disappearance of the stimulus. Apparently a complex of perceptual and attentive factors, in addition to position in the visual field, determine whether the stimulus is visible. Complete analysis of the factors is not readily possible, but such things as rate of movement, unsteadiness of the stimulus, and deliberate movement back and forth are complicating items.

From our trained observers 25 maps apiece were obtained with each direction of movement. Figure 1 shows a set of measurements made with one O. In Figure 2 the squares show the average of the right eyes of all O's for the limits of sensitivity obtained separately by moving the stimulus <u>in</u>, and by moving it <u>out</u>. The scotomata obtained by movement of the stimulus in from the periphery are about 1/3 the diameter, or 1/10 the area, of those obtained by moving it out from the insensitive region. The variability with the outward movement $\sigma = .87^\circ$ is somewhat larger than for the inward movement $\sigma = .63^\circ$. If we take an average of the in and out determinations we get the larger scotoma

shown in Figure 3, but the standard deviation is 1.55°. Obviously, neither procedure gives a satisfactory answer as to the size of the foveal blind spot, or provides a basis for setting up a test using the area of this region as a measure of night vision.

Because of these difficulties, we eliminated movement of the stimulus. We presented the stimulus at certain positions along the radii for two seconds at any position. The observer reported simply whether the stimulus was visible or not visible. Two serial orders were maintained, beginning in the periphery and in the central region. This gave us a check on the influence of the previous visibility of the stimulus as a factor in determining its perception at the next step. In Fig. 2 the circles show the scotomata with discrete stimulation. Differences between "in" and "out" series are negligible for our purposes. Also, discrete presentation eliminates much of the variability present in the results of the moving stimulus procedures as indicated by the smaller σ. These are for separate directions for both eyes, in $\sigma = .61°$, out $\sigma = .57°$ and for the total of 100 determinations $\sigma = .61°$ as against $\sigma = 1.55°$ for the moving stimulus.

The absolute size of the scotoma thus obtained lies between the limits of the two areas obtained by moving the stimulus, but because of the lower variability the area is defined much more precisely than it is by the cruder method of moving the stimulus. In this finding we are in agreement with the recommendation made by Ferree and Rand that movement of the stimulus be avoided in all perimetric measurements.

Since we may take the limit of the scotoma to be a sensitivity gradient, the discrete presentation method gives a more satisfactory determination of the position in this gradient of the line of demarcation of sensitivity to a stimulus of a given size and intensity.

Though we are still far from setting up a test, our data point to certain possibilities. If the extent of the scotopically insensitive area is to be used as a test of low level sensitivity, a device may be worked out with which to present a number of discrete points along chosen radii. The maintenance of fixation is still crucial, but with stimuli at a scotopic level the subject would be penalized if he did not maintain fixation. Such a test would take account of the sensitivity gradient in all direction from the fovea, and it may show that peripheral sensitivity is more definitive than central blindness.

II

My second item concerns "off center fixation". The use of off center fixation for scotopic viewing is well known. However, the precise amount and direction of the "off center" seems to differ from problem to problem without any indication of a common basis. Several of the better known tests for night vision use 5° up from the target.

A short study by Louise Sloan Rowland is aimed at this problem. Her data show a peak in acuity at about 4° on the nasal side of the fixation point (temporal retina) at illuminations from about 6.3 log µµL down to 4.3 log µµL. Data were taken out only to 10° on the temporal side. She concludes, "This indicates that for the eye of the particular subject studied, fixation 4° temporal to the object viewed is most favorable for form discrimination at scotopic intensity levels." No account is taken of the over-all size of the target.

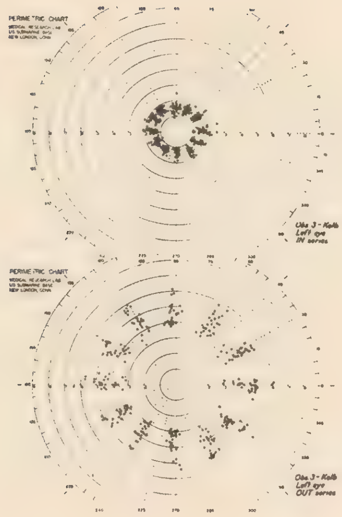


Fig. 1

Sample of data from one observer.

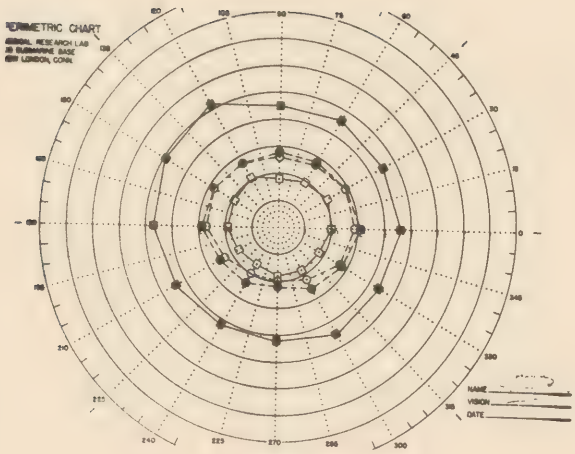


Fig. 2

Average determinations for all observers - right eye

Solid squares - moving out
Open squares - moving in
Discrete presentation: solid circles - out series
                        open circles - in series.

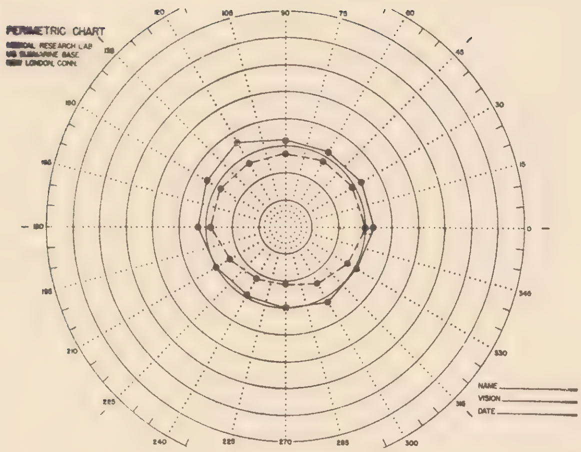


Fig. 3

Average of all observers - right eye

Dotted line - discrete presentation
Solid line - moving stimulus

This conclusion agrees with general practice so far as the distance is concerned, but not in direction. She did not examine the vertical direction which is frequently used. Moreover, we believe that her data do not justify so limited an interpretation because sensitivity appears to fall off quite slowly from the 4° point out, and for brightness intensities near 4.6 log µµL, does not change appreciably from 4° to 30°. The indication is, then, that fixation should be beyond 4° depending upon the size of the target, so that the distance from the fixation point to the nearest edge of the target should be greater than 4°.

In our experimental work with acuity at low illumination we found that this angle is an important factor. Since we wish to use the same type of target at all levels of illumination so that we can define acuity in terms of the angular subtense of the minimum units in the target area, it is necessary to have our target of a considerable total angular subtense. We found that targets consisting of units of the given subtense, extending from 4° to 8° in the field were much less well-perceived than targets with the same units extending from 5° to 20°. Since we are interested in using the best conditions for scotopic acuity, the latter size of target has been adopted. This choice also gives better control of the fixation factor. With the smaller targets the observer could give more accurate and more certain judgments if he moved his fixation away from the fixation point so that the checkerboard fell on a more peripheral area. With the large targets, no such improvement results.

Since the target is presented at various positions in the visual field around the fixation point, the effect of the blind spot must be taken into consideration. When the checkerboard is in the blind spot position the sensitive area stimulated is reduced. This reduction of the effective area of the target may give us indication of the effect of the areal factor if at critical sizes of target the reduction in total area reduces visibility of the target.

III

Our third point has to do with the brightness gradients and the angular gradients at low levels of illumination.

For ophthalmological purposes, measurements of acuity in regions far from so-called normal or 20/20 vision have found large steps of angular subtense adequate. Usually these were units of 1/4 to 1 $\log_2$ steps. Similarly, ordinary practice seemed to indicate that appreciable changes in intensities would fall into $\log_{10}$ steps.

Our experimental aim has been to determine at a given intensity the way in which acuity changes as measured in terms of the angular subtense of the target units. To do this, we present at a given brightness level a succession of targets in which the angular subtense of the checkerboard units varies from one that gives 100% resolution to one that gives 0% resolution. The first indication of our experimental results is that the acuity change which we desire to measure is more sensitive than can be adequately determined with the conventional 1/4 $\log_2$ units. The latter have been reduced to 1/8 log steps and in some critical regions may have to be reduced still further. The course of our results may furnish a basis for questioning the validity of a log scale for this type of experiment.

A similar discrepancy between test units and experimental units seems

to occur with the intensive steps. Within 1/2 $\log_{10}$ unit, for example, from 4.5 log uuL to 4 log uuL the visibility of a given size of target changes from 100% to 0%. It appears, therefore, that scotopic acuity for large areas of low contrast is more sensitive to brightness change than had been assumed, and that it must be evaluated at relatively small gradations through crucial regions. Steps of intensity that are not obviously noticeable may, nevertheless, alter acuity.

Discussion

Dr. Sloan commented that her work to which Dr. Dimmick referred, was based upon only one subject. Dr. Sloan reported that an additional paper exists in which additional subjects were used, and in which measurements were made in both the horizontal and vertical dimensions.

Dr. Dimmick commented on one aspect of the work which he had not emphasized. He reported that there was evidence of lower probability of correct detection when the checkerboard pattern fell on the physiological blind spot. Presumably, this is equivalent to a reduction in the size of the visual acuity object.

Dr. Scobee asked whether Dr. Dimmick believed that a test of the apparent size of the central scotoma would make a feasible test for night vision. He asked whether the variations obtained from test to test would not render the measurement of little clinical value.

Dr. Dimmick stated that he was not certain, but that data being obtained should yield the answer.

Dr. Dimmick commented on the apparent increase in acuity which was obtained by going further into the periphery than 30° from the fovea. It was agreed that because of the changing rod and cone distribution as a function of location in the periphery, there could be an increase in acuity, provided the acuity discrimination was based upon a brightness discrimination.

Dr. Fry asked what type of test object was used in Dr. Sloan's study in which a continuous decrease in acuity was noted for increasingly peripheral locations.

Dr. Sloan replied that the broken circle test object was used.

Dr. Rand asked whether Dr. Dimmick obtained evidence of significant distortions from a circular central scotoma.

Dr. Dimmick replied that the data indicated amazingly little distortion of circular scotomas, provided sufficient data were averaged to eliminate sampling error.

In answer to a question from Dr. Sulzman, Dr. Dimmick stated his general belief that a measure of parafoveal sensitivity is more significant than a measure of central blindness for night vision testing.

Dr. Sulzman pointed out several differences between the technique used by Dr. Dimmick and that used by Livingston in scotometry. He wondered to what degree the two techniques were comparable.

Commander Farnsworth emphasized the magnitude of the sampling error which normally exists in measurements of central scotoma. He pointed out that Livingston's data often indicate peculiarly distorted scotomas which were apparently assumed by Livingston to represent real distortions in central sensitivity. When sufficient data were collected, Dr. Dimmick showed that the central scotoma was circular in form.

Commander Farnsworth concluded, therefore, that data on the shape of central scotoma obtained with small samples have practically no significance.

REPORT TO ARMY-NAVY-NRC VISION COMMITTEE CONCERNING COMMANDER FARNSWORTH'S

REPORT ON THE CORRECTIVE TRAINING OF COLOR BLINDNESS

At a meeting in Chicago, October 14, 1946, of the American Committee on Opitcs and Visual Physiology, correspondence was discussed from Dr. Blackwell of the Army-Navy-NRC Vision Committee, Commander Farnsworth, and Dr. Berens regarding the Dvorine method for corrective retraining of color blindness. It was voted at that time to ask Commander Farnsworth to conduct an investigation on this subject.

Commander Farnsworth completed this report in July, 1947. This report was accepted by the committee and at an executive session of the component societies (The Section on Ophthalmology of the American Medical Association, The American Academy of Ophthalmology and Otolaryngology, and the American Ophthalmological Society) the broadest possible publicity for the report was authorized. The complete report was published in SIGHT-SAVING REVIEW, VOL. XVII, No. 4, appearing in March, 1948. A more popular version entitled, PATENT MEDICINE FOR COLOR BLINDNESS, has been prepared by Commander Farnsworth and accepted for publication in HYGEIA. Also, Dr. Fishbein has written us that an editorial based on the report has been prepared for the J. A. M. A.

A formal report has been prepared and distributed by the Medical Research Laboratory, United States Submarine Base, New London, Connecticut. The title is "AN INVESTIGATION OF CORRECTIVE TRAINING OF COLOR BLINDNESS, Color Vision Report No. 15, 15 July 1947. In conclusion Commander Farnsworth states:

> "The opinions of nearly every authority on color vision in America are represented in the above communications. They indicate that the best informed and most experienced specialists in the field of color vision are emphatically of the belief that congenital color deficiency cannot be remedied by the use of diet, medicine, training or other treatment now known to science.
>
> "The major question has now been answered as a whole. An explanation of conflicting interpretations of experimental data may be found in the following statements which are selected with the idea of separating various phases of the problem more distinctly.
>
> "1. The basic psycho-physiological functions, as indicated by luminosity curves, color mixture ratios and other stimulus data by which normal or defective color vision is described, are unaffected by medicine, training or other therapy.
>
> "2. Practice and coaching will undoubtedly enable a color deficient person to pass, or to show an improved score on, an imperfect test.
>
> "3. But there is no well established proof that training a person to pass a color-blind test contributes to rehabilitation in the true sense of the word, because the skills developed have no practical value except that of defeating the purpose of the screening test.
>
> "4. The only aspect of color-blindness that can probably be modified

by training methods is the ability to differentiate chromas, and the tests used for measuring improvement should concentrate on this aspect of the problem.

"5. Improvement measured by such means could not be interpreted as a claim to have made changes in the other and more basic aspects of color-blindness.

"6. A program formulated on these principles would also be of value in training persons with normal color vision to achieve a finer discrimination of colors.

"(Paragraphs 3, 4, 5, and 6 are in the words of Glenn A. Fry, Director, School of Optometry, Ohio State University, from "Rehabilitation of the Color Blind", Michigan Optometrist, November, 1943.)"

Respectfully submitted,

Conrad Berens, M. D., Secretary
For the American Committee on Optics and Visual Physiology
Thomas D. Allen
S. Judd Beach
Frederick C. Cordes
Alfred Cowan
Walter B. Lancaster
Lawrence T. Post
Avery D. Prangen
Kenneth C. Swan

SUBCOMMITTEE ON COLOR VISION

The specific reason for the formation of this Subcommittee was a recommendation by the Committee on Ophthalmology, National Research Council, at its meeting of 6 December 1947 that (1) the use of all Edridge-Green Lanterns now in use by the Navy be discontinued, and that they be replaced by the School of Aviation medicine color threshold tester (SAM CTT), (2) that examinees failing the color test currently in use (selection of American Optical Company's Pseudo-isochromatic Plates) be further tested with the color threshold tester, and rated as Grades I, II, III or IV on the basis of the scores thus obtained, (3) that examinees rated as grade IV shall be considered as disqualified for service under normal peacetime conditions, but that (4) a certain number of men rated as Grade IV color vision be admitted to the Navy and their records concerning disability from color vision be followed, to provide information on the value of color-blind men in the Navy.

The Subcommittee was organized as follows:

Dr. D. B. Judd (Chairman)
Dr. A. Chapanis
Lt. Cmdr. Dean Farnsworth
Dr. Gertrude Rand
Dr. Louise Sloan

Its first meeting was held on February 27, with four representatives of the Bureau of Naval Personnel, five representatives of the Bureau of Medicine and Surgery, three representatives of the Office of Naval Research, and one representative of the Air Surgeon, present during at least part of the proceedings. There were two main items on the agenda:

1. The problem of the admission of color deficient men in the Naval Service and plans for a follow-up study throughout the term of their enlistment, and

2. The validation and field testing of the Navy Color Lantern.

After extended discussion the Subcommittee agreed upon recommendations supporting those by the NRC Committee on Ophthalmology as follows: (1) that the use of the Edridge-Green Lantern be discontinued, (2) that the AO plates and AAF SAM CTT be used, (3) that color-defective men be admitted to the Navy, and (4) that studies be made of the degree to which color blindness interferes with the performance of duties in the Navy. The Subcommittee, however, departed from the NRC Committee recommendations (1) by recommending a battery of six instead of two color-vision tests (addition of the Hardy-Rand-Rittler Polychromatic Plates, Navy Color Lantern, Anomaloscope, and Farnsworth Dichotomous Test), and (2) by recommending job analyses by studies of carefully controlled groups of men, both of normal and color-defective vision, instead of career-following of all color defectives. This disposed of the first item on the agenda.

It was further recommended that a correlation be obtained between the AAF SAM CTT and the Navy Color Lantern together with a test-retest of each, plus a pseudoisochromatic test for the purpose of selection of subjects, specifying a minimum acuity of 20/40, and illumination of the plates by means of a standard Macbeth easel lamp. This ended formal consideration of the second item on the agenda. At the request of the chairman, however,

Lt. Cmdr. Farnsworth prepared and submitted, by letter, further detailed suggestions for the above correlations and tests. These were reviewed and approved by the Chairman.

A third item arising from the Bureau of Ordnance was presented by Dr. Imus for consideration by the Subcommittee. It has to do with the specification of the red illumination of instrument dials to be read by dark-adapted personnel in the attack center of submarines, and with the specification of photometric brightnesses of self-luminous areas of other colors (pointers, dial numbers, scale divisions, and so forth) such that immediate identification of the dial would be facilitated without injuring the dark adaptation of the personnel.

It was brought out in discussion of this problem that the interference to dark adaptation of a given portion of the visual field can be measured directly by the corresponding photometric brightness determined according to the luminosity function for the dark adapted eye (the red luminosity function). Furthermore, the stimulating power of a self-luminous area for form vision required to yield legible instrument indications, is measured directly by photometric brightness, which is determined according to the luminosity function of the light adapted eye (the cone luminosity function). The Subcommittee recommended that the Chairman submit, in accordance with these principles, a restatement of the specifications for the red light to be used in dial illumination; this has been done. The Subcommittee also recommended that the balance of the problem dealing with lights of other chromaticities than red be referred to the special working committee of the Vision Committee which is being organized to give practical answers to these questions for the Chief of Ordnance of the Army and for the Bureau of Ordnance. (Subcommittee on illumination)

Background information on color-vision testing in general, and a bibliography of publications dealing with color-vision tests by the Army Air Forces and the Bureau of Medicine and Surgery, have been supplied by the Chairman to Dr. Ross A. McFarland, Division of Research, Graduate School of Business Administration, Soldiers Field, Boston, who is making a study of color-vision requirements for pilots of commercial and private planes.

This completes the summary of what the Subcommittee has done.

The Subcommittee on Color Vision has been organized with the understanding that no large-scale demands on the time of the members is in prospect. It is expected that the Subcommittee will meet only when urgent problems in color vision arising in the services are assigned to it. It is hoped that the deliberations of the Subcommittee will require only one working day each year. The members of the Subcommittee, like members of the organizations employing them, are participating more or less actively in the further validation and field testing of the Navy Color Lanterns, but it is expected that no research will be undertaken by this Subcommittee on its own initiative.

D. B. Judd, Chairman

THE USE OF THE NAVY LANTERN IN A COORDINATED COLOR PROGRAM FOR THE ARMED FORCES

Dean Farnsworth, Lt. Comdr. H(S) USNR

It would be highly desirable if color coding could be entirely eliminated. However, it seems impossible to avoid some use of color, at least for signalling purposes. Moreover, new uses for color are always arising. We therefore wish to know what colors may be used.

Less than one out of one hundred thousand persons is yellow-blue blind. Accordingly, we can safely use white, amber, and blue, but only at large subtense, since the fovea is yellow-blue blind. However, since most color signals are, or may be, point sources, subtending small visual angles, the red, white, and green ranges must be used for coding in the majority of situations.

About 9% of men are color abnormal. Some 6% are highly defective; they cannot be used in a service demanding color discrimination. The remaining 3% can be put into service only if the standard color signals are made readily distinguishable to this group.

The problem has been attacked[1] of what green, white, and red are most distinguishable to this 3%. A slightly orangish-red, and incandescent-lamp white, and a bluish-green were found which were distinguishable by the 3% and by normals at small subtense. Because they contain different amounts of yellowness and blueness, they are readily distinguishable by partial color defectives, but because they are in the red, white, and green zones they are also distinguishable at small subtense by normals.

As a matter of fact, most red and green signal lights are now specified to meet these requirements,[2] and color defectives are usually correct in saying they almost never make a mistake.

What test, then, should be devised for this 3%? Obviously, one within the region of most confusable colors. It is this region which the Navy Lantern is designed to test.[3] The most confusable red, green, and white hues are used, at moderate intensities, with small, but not pin-point, apertures, at high saturations; the two lights of any pair are mismatched in brightness. Thus, the test represents general field conditions.

The Navy Lantern, then, was built as a complement to the proposed specifications for the most distinguishable signal lights. It is assumed that if a man can pass the Navy Lantern, containing the most difficult set of red, green, and white colors, he can be trusted to distinguish the easiest set of red, green, and white signals, with a safety margin in case the signals themselves are not seen under optimum conditions.

The borderline side of this 3% group, so mildly color deficient that different types of tests give different results, is especially troublesome to the Armed Forces. Use of the proposed signal lights and of testing with the Navy Lantern would enable us to utilize this slightly color-weak group.

References

1 Dean Farnsworth, "Confusions of Colored Lights at Small Subtense by Protans and Deutans," BuMed Project No. X-263 (Av-151-c), Color Vision Report No. 13, U. S. Naval Submarine Base, New London.

2 Dean Farnsworth, "Proposed Specifications of Red and Green Navy Signal Lights," BuMed Project No. X-265 (Av-153-c), Color Vision Report No. 16 (Revised 1 March 1948), U. S. Naval Submarine Base, New London.

3 Dean Farnsworth and Priscilla Foreman, "Development and Trial of New London Navy Lantern as a Selection Test for Serviceable Color Vision," BuMed Project No. X-457 (Av-241-k), Color Vision Report No. 12, U. S. Naval Submarine Base, New London.

Discussion

Dr. Berens asked whether the Navy Lantern could not be adapted for use in viewing distances closer than 7 feet by changing the lens. He suggested making the color lantern similar, in this way, to the Ortho-rater.

Commander Farnsworth reported that it had just occurred to him, also, that it would be possible to introduce a lens which would make the instrument useable at a short distance.

Dr. Rand asked whether the addition of a lens might not distort the results of the lantern through action of chromatic aberration. The presence of chromatic aberration introduced by the lens would increase the apparent size of the image in the test.

Commander Farnsworth replied that the difference in apparent size for different colored lights is "apparent and not real". He stated that the difference in brightness associated with the difference in size is normally a clue which the color deficient uses in making "color" discriminations. Commander Farnsworth stated that since a brightness difference of 20% was introduced into the test in order to confuse the color deficient, additional chromatic aberration effects would merely magnify the clues available to the color deficient in making erroneous judgment.

Dr. Marquis asked Commander Farnsworth about the re-test reliability of the Navy Lantern.

Commander Farnsworth replied that re-tests on approximately 2,000 people resulted in three misclassifications on the re-test.

Dr. Uhlaner questioned Commander Farnsworth's conclusions concerning field validation studies.

Dr. Sloan reported that at Randolph Field, very satisfactory validation of color discrimination tests was obtained in tests with pyrotechnic signals. She reported a clean-cut discrimination in the degree of recognition of flare color between the color normals and the color deficients.

Commander Farnsworth replied that, actually, a study such as the Randolph

Field study was only a limited validation, since the field conditions were so very similar to the conditions of the test used. He agreed that since there are many jobs in the military service which require different kinds of color abilities, complete validation would not be feasible. Commander Farnsworth continued by pointing out that it was sufficient for many service uses to separate the extremely color deficient from all others, and that differentiation between normals and near-normals was relatively unimportant.

Synopsis of Comments by Dr. S. S. Ballard on the subject of

INFRARED AND ULTRAVIOLET TRANSMISSION OF MILITARY SUNGLASSES

The discussion following the paper given at the twentieth meeting by Dr. Alphonse Chapanis, "Recommendations for the Use of Sunglasses in the Armed Forces," indicated that there was still some lack of unanimity of opinion regarding the ultraviolet and infrared protection provided by current military sunglasses. This discussion, of course, refers to the general-purpose type of goggle rather than specialized sun-scanning goggles through which the disk of the sun is directly viewed. A search of the minutes of previous Vision Committee meetings indicates that the following is the present state of affairs:

Investigations by Drs. Blum, Peckham and others have shown that the transmission characteristics of current military sunglasses (whose lenses are standardized on the 12 percent transmission neutral polarizing plastic sheet material) offer adequate protection at both ends of the visible spectrum. This plastic lens cuts off at about 3000 angstroms in the ultraviolet, just above the erythemal threshold. Although it does transmit in the infrared, there is no body of evidence to indicate that these rays are harmful, with the exception that Dr. Hecht has established that too bright daylight illumination has a distinctly deleterious effect on subsequent dark adaptation for night vision. In view of the above, it appears that there is no cause for alarm in regard to the spectral transmission of current military sunglasses. The best references in this connection are probably H. F. Blum 2,4 and R. H. Peckham 7,9.

It was pointed out that there is now available an ultraviolet absorbing dye which can be incorporated into plastic sunglass lenses, and which will prevent the transmission of practically all radiation of wavelength shorter than 4000 angstroms. This dye is said to be used currently in certain commercial goggles, but has not yet been specified for military goggles.

It was also stated that a surface-hardening technique is now available which will render plastic sunglass lenses 10 to 20 times as scratch-resistant as the untreated lenses. This evaluation is based on standardized tests which simulate service conditions (the abrasion that comes from cleaning fine dust from a plastic lens with a handkerchief, taking goggles in and out of their carrying case, etc.). The tests ordinarily applied here are the Taber Abraser test or the A. S. T. M. dropping carborundum test. It should be noted that the surface-hardened plastics are still as liable to scratching by sand, pointed objects such as knife blades, etc., as are the ordinary plastics.

The following abridged bibliography is submitted for reference and discussion on Military Sunglasses:

1. Schroeder, Peckham, Korb: General Purpose Sunglasses (18 May 1944), 2 : 7,8.

2. Blum, H. F.: Protection Against Excessive Solar Radiation (18 May 1944), 2 : 13-20.

3. Hecht, S., Hartline and Friedenwald: Protection Against Injury from Solar Radiation (16 June 1944), 3 : 16-21.

4. Blum, H. F.: Solar Energy Reaching the Retina: Proposed Spectral Curve for Testing Sun-scanning Glasses (27 July 1944), 4 : 13-21.

5. Toucey, R. M.: Sunglasses (Questions as a Guide to Procurement) (27 July 1944), 4 : 23.

6. Toucey, R. M.: Proposed Standardized Sunglass Design (16 September 1944), 5 : 9-11.

7. Peckham, R. H.: Solar Transmission of Glass and Plastic used in Sunglasses (16 September 1944) 5 : 12-28.

8. Hecht, S.: Influence of Sunlight on Night Vision (12 October 1944), 6 : 16-20.

9. Peckham, R. H.: The Present Status of Goggles and Sunglasses in the U. S. Navy (13 March 1945), 10 : 10, 11.

10. Miles, W. R. & Committee: Report of the Sub-Committee on Sunglasses (12 June 1945) 12 : 40-55.

11. Chapanis, A.: Recommendations for Use of Sunglasses in the Armed Services (20-21 October 1947), 20 : 93-97.

lr/hlw
5/26/48

Discussion

Commander Farnsworth commented upon Dr. Ballard's discussion of sunglasses. Below we have transcribed a statement prepared by Commander Farnsworth summarizing his remarks:

"I am convinced that the most desirable goggle for Service use is a lightweight, comfortable plastic. I have always found the Polaroid Corporation most sincere and capable in their efforts to produce increasingly better optical devices. My comments are made in hopes of stimulating improvements.

"1. With regard to ultra-violet: the transmittance of the Navy Brow-rest is not high in terms of percent transmitted, but it is high in comparison with other good sunglasses. Our most recent set of tests for ultraviolet transmittance was run by E.T.L. on 9 standard brand goggles and the Polaroid brow-rest showed the highest transmission of ultra-violet of any in the group.

"2. Total transmission in the visible region is agreeably low; some of the filters appear neutral by inspection and the curves presented do not appear to be highly dichroic. However, the misleading curves supplied to the Navy in 1944 for the HN-12d goggles (there is no question but that they were supplied in good faith) emphasize the need for being wary of spectrophotometric curves on polarizing materials unless accompanied by a description of the techniques employed.

"3. The question of how much ultra-violet is tolerable is still open, but there is no doubt of the discomfort occasioned by heat. The dangerous region is generally believed to be the first octave beyond the visible. Our laboratory tests show very high transmittance, between 60% and 80%, in this infra-red band for Polaroid goggles.

"It appears that the Vision Committee is under a misapprehension if it believes that the Polaroid brow-rest goggles met low specifications for ultra-violet and heat transmission."

MINUTES AND PROCEEDINGS OF THE SECOND MEETING OF

THE SUBCOMMITTEE ON VISIBILITY & ATMOSPHERIC OPTICS

A meeting of the Subcommittee on Visibility and Atmospheric Optics was held May 26, at the U. S. Naval Submarine Base, New London, Connecticut. The following were present:

Dr. E. O. Hulburt, Chairman
Dr. H. Richard Blackwell
Dr. Howard S. Coleman
Dr. S. Q. Duntley
Dr. Arthur C. Hardy
Dr. H. K. Hartline
Dr. E. S. Lamar
Dr. Lorrin A. Riggs
Dr. Richard Tousey
Mr. W.E.K. Middleton
Mr. Michael Goldberg
Commander D.R.E. Brown
Mrs. Elizabeth Kelly

Dr. Hulburt opened the meeting and suggested that the agenda consist of reports from the various working groups concerning their progress in preparing the technical reports called for at the first meeting of the Subcommittee. The reports of the various working groups, and the discussions are contained in the following pages.

Section I-A

ATMOSPHERIC ATTENUATION

Working Group:
Dr. Howard S. Coleman, Chairman
Mr. W.E.K. Middleton
Dr. S. Q. Duntley

Dr. Coleman reported to Dr. Hulburt that, although the working group has not met formally, "there has been a number of informal conversations which led to some fairly specific suggestions for research." Dr. Coleman listed them as follows:

"(1) There is a need for further contrast attenuation measurements along a horizontal path. This would include the determination of the contrast attenuation of black targets and white targets as a function of the target distance, azimuth of the target with respect to the sun, weather conditions, vertical and horizontal illumination along "the line of sight", and for different types of terrain with special consideration being given to hot desert regions, salt water bodies, and foggy regions.

"(2) The attenuation of contrast along slant heights should be explored.

"(3) There is a particular need for a 'visibility meter' by means of which

the meteorological range can be determined. It would be desirable to have such a device as small and light in weight as possible.

"(4) There is a need for further development of procedures by means of which it would be possible to compute visibility (recognition and detection) of objects viewed through optical instruments. Likely the most useful procedure would be one which makes use of certain laboratory measurements made on the optical instrument in question and which would make allownaces for the response of the human eye and the influence of the atmosphere on such observations.

"(5) There is a need for the study of the reduction in contrast by the atmosphere along horizontal paths of sight for the case of targets brighter than their background. It has been suggested that there should be a limiting contrast above which Koschmeider's Law must fail. If this be the case, it would be desirable to determine the magnitude of this limit.

"(6) There is a need for exploring the luminous density as a function of altitude. This would be particularly important in building into any visibility prediction scheme a quantity which would make allowances for adaptation level of the human eye at the higher altitudes. It is my understanding that there is a possibility that the Navy Department may be able to make available B-29's for this purpose, and that it is possible that Dr. S. Q. Duntley will propose to do this sometime next fall.

"(7) Some consideration should be given to the need for an experimental study of the relation between beam transmittance through the atmosphere and the contrast transmittance over the same path. It is probable that such a study would be essential in the development of a 'visibility meter' of the type mentioned above."

Dr. Coleman reported that he might be unable to attend the Subcommittee meeting, and asked that the following two items be brought up for discussion:

"The first deals with the resolution recently passed by the SubCommittee on the desirability of making continuous measurements for the period of two years of the type recently being made at The Pennsylvania State College on Atmospheric Attenuation. In my opinion this work is highly desirable and would lead to definite advances in our knowledge of atmospheric optics. However, it should be pointed out that such a program would be fairly costly if it were to be conducted at three or four locations and if it were to include a study of such factors as are mentioned in the first numbered paragraph above. A rough estimate shows that the probable cost would be of the order of $250,000. to $500,000. to complete such a program. A second matter which I believe should be given special attention is the possibility of getting such visibility data as the Tiffany Nomographic Charts into more hands than is possible at the present time with the present limited supply. A great many people have evidenced interest in receiving copies of such charts. Perhaps it would be in order for the Vision Committee to suggest the publishing of such charts for relatively wide distribution.

"In addition to the above information, perhaps it may be of interest to report that the work started at the Pennsylvania State College on the measurement of atmospheric attenuation is to be conducted at The University of Texas under an ONR contract."

Discussion

Dr. Coleman reported on his plans for continuing measurements of atmospheric attenuation at the University of Texas.

Dr. Duntley called attention to an error in the report of the working group, under Paragraph 6. Dr. Duntley stated that: "The need for luminous density exploration arises from the fact that the present contrast attenuation equations for inclined paths of sight contain the assumption that luminous density is independent of altitude. This assumption was put into the theory not of necessity but because it was the experimental finding of The Tiffany Foundation. I am convinced of the general truth of the Tiffany finding, but I believe it is highly important to carry the exploration to higher altitudes, and to determine how low the sun may be before variations are encountered."

Mr. Middleton discussed the three types of visibility meters which he has arranged to borrow from England for testing in the Roscommon experiments.

After considerable discussion of visibility meters in general, it was agreed that the Roscommon tests should provide some measure of the degree of validity of various types of instruments.

It was agreed that the recent publication of nomographic charts in the J.O.S.A. article by Dr. Duntley should take care of the general need for the material. Dr. Blackwell reported that reprints are available from the Vision Committee Secretariat upon request.

Section I-B

Atmospheric Refractive Phenomena

Working Group: Dr. Hans Neuberger
Mr. L. P. Harrison
Mr. C. A. Douglas

On 13 March 1948, the members of the working group convened with Drs. H. R. Blackwell, P. O. Huss, and Mr. D. I. Portman at Akron, Ohio, in order to confer on the following matters in connection with the Akron-Scintillation project:

(a) Optical and meteorological instruments and equipment to be installed;
(b) Design of observational procedures;
(c) Selection of meteorological conditions under which observations are to be made.

The members of the working group remained in Akron on 14 March for the purpose of preparing an outline of the summary of facts and theories pertaining to atmospheric refractive phenomena.

A copy of this outline follows.

The outline was tentatively divided into two parts, one to be prepared in due time by L. P. Harrison, the other one by H. Neuberger.

A list of over fifty literature references containing works in English, French, Italian, and German has been compiled so far.

OUTLINE OF REPORT ON ATMOSPHERIC REFRACTION PHENOMENA

1.0 Refraction Phenomena in the Atmosphere with Average Vertical Density Gradient

1.1 Fundamentals of Atmospheric Refraction

- 1.11 Formula for refractive index (p.61)* (r.n. sin) = const.)
- 1.12 Table of refractive index for various wave lengths
- 1.13 Formula of refractive index as function of T, p, e.
- 1.14 Formula of light path (p.65)*

1.2 Astronomical Refraction

- 1.21 Effects on elevation of heavenly bodies
  - 1.211 Sun -- daylength -- latitudinal variations
  - 1.212 Moon
  - 1.213 Stars
- 1.22 Deformation effects
  - 1.221 Sun and Moon
- 1.23 Dispersion effects
  - 1.231 Green flash (probable)
  - 1.232 Edge effects (on stars, sun, etc.; too small (refer to exceptions in scintillation) (p.71) Table

1.3 Terrestrial Refraction

- 1.31 Effects on elevation (function of distance, also special cases treated later) Table p. 76 (average lapse rates and water-vapor gradient)
- 1.32 Depression of the horizon (as a function of height, order of magnitude, table p. 77, 79 (special conditions)
- 1.33 Lateral effects (case of sighting past heated objects or icebergs)

2.0 Refraction Phenomena in the Atmosphere with Special Vertical Density Gradients

2.1 Differential aspects of subtended angle and elevation

- 2.11 Towering
- 2.12 Stooping
- 2.13 Looming
- 2.14 Sinking

2.2 Mirages (General Discussion) (refer to p. 152)

- 2.21 Upward mirages

---

* Page numbers refer to Pernter-Exner "Meteorologische Optik", Second edition.

2.22 Downward mirages
2.23 Fata Morgana (artic)
2.24 Deformation of sun and moon
2.25 Lateral mirages

3.0 Scintillation

3.1 Introduction (Variation in position, brightness, color)

3.2 Astronomical Scintillation

3.21 General: (Aperture of receiver and subtense of source; source elevation; meteorological conditions; frequency and amplitude of vibrations; variations in flux and color; consequently apparent variation in intensity: terrestrial influence such as of mountains)
3.22 Fixed stars
3.23 Planets
3.24 Sun and Moon (edge effect)
3.25 Moving shadows (sun, eclipse)

3.3 Terrestrial Scintillation

3.31 General Problem:
3.311 Source of illumination, steady vs. flashing sources, and objects' characteristics (aperture)
3.312 Geometrical arrangement (moving shadows over white reflecting surface; relative to sun, object, observer positions; size, distance, elevation)
3.313 Atmospheric conditions
3.314 Receiver and its characteristics (optical aids, telescopes, photocell, time constant of receiver, aperture) periodic time of air movement and spacing of air parcels
3.315 Underlying surface (its variability and adjacent surfaces)
3.32 Target
3.321 Nighttime
3.3211 Luminous sources
3.32111 point sources
3.32112 extended sources (effect of size)
3.32113 combinations of point and extended sources
3.3212 Nonluminescent sources
3.32121 specularly reflecting sources (aperture)
3.32122 diffusely reflecting sources (aperture)
3.3213 Combination of luminous and nonluminescent sources
3.322 Daytime
3.3221 Luminous sources
3.32211 point
3.32212 extended (size)
3.3222 nonluminescent sources
3.32221 specularly reflecting (size)
3.32222 diffusely reflecting sources (size albedo, angle of illumination, type of source, parallel, diffuse, sun, sky, etc.; spectophotometric properties probably of second order)

3.33 Source of illumination (as producing general field of flux; relative magnitude of target brightness)

3.331 Natural: sun, moon, aurora, sky, etc.; surface reflection.

3.332 Artificial:

3.3321 Collimated sources

3.3322 Diffuse sources

3.34 Special Aspects

3.341 Scale

3.342 Geometric orientation (angle between observer, light, and target)

3.35 Atmospheric conditions ((temperature differences (lapse rates), advection, K-and W air masses, convection, humidity schlieren, small scale convergence and divergence (gustiness), shape of parcels (cylindrical etc.) dependent on vertical gradient of wind and temperature; wind structure; convective clouds and precipitation as governing lapse rate and convection; solar radiation as affecting lapse rate near surface; immediately antecedent modifying influences on air masses including terrain; katabatic and anabatic motion; local winds; terrain differences (coasts; icebergs; glaciers; incoherent snow cover; grass-plowed field-highways; town areas; artificial sources such as kiln, stacks, etc.; thermal fog dispersals); cloudiness and fog as related to air mass properties; optical effects and physical effects of droplets in the air))

3.36 Receiver

3.361 Human eye

3.3611 unaided

3.3612 aided eye

3.362 Photographic receivers

3.3621 still picture

3.3622 movie (time lapse, high-speed)

3.363 Photoelectric receivers (conductive, emmissive types, also mention thermal types)

3.37 Underlying surface (not just line of sight)

3.371 Physical properties

3.3711 absorptive and radiative properties

3.3712 reflective properties

3.3713 conduction

3.3714 latent heat due to H_2O (change in phase, vegetation)

3.3715 surface character (roughness, form, nature, wooded, vegetated surfaces; mineral surfaces; H_2O phases

3.372 Distribution of surfaces of different physical properties

3.3721 Uniform

3.3722 Nonuniform

3.4 Theory of scintillation

3.41 Astronomical

3.42 Terrestrial ($\frac{s = \text{pathlength}}{c \text{ speed of light}}$ as a fundamental unit of time; periodicity of gusts; mean free path between parcels. Random and translatory motion in transport velocity.)

4.0 Refraction, Diffraction, Reflection involving atmospheric suspensoids.

4.1 General: (Halos, Coronas, their significance restricted to occasion when forming background of visibility targets or when extending the angle of subtense of terrestrial light sources)

4.2 Halos (refer to theories existing: optical processes involving ice crystals)
    4.21 Halos produced by refraction
    4.22 Halos produced by reflection
4.3 Coronas (optical processes involving any suspensions of sufficiently small size)
    4.31 Phenomena caused by diffraction
    4.32 Phenomena caused by diffraction and reflection
4.4 Rainbow (optical processes involving water droplets)
    4.41 Direct rainbows
    4.42 Reflected rainbows
    4.43 Fog bows

5.0 Summary. Summarization of aspects pertaining to visual range, discussed in 1.32, 1.33, all of 2.0. Also 3.0, specifically range finding accuracy; field tests of laboratory data disturbed; heat fogs, especially over high plateaus, signalling (challenging of stars, position lanterns, use of surveying instruments, telescopes, binoculars for identification, scanning, detection. Also 4.0, haloes etc; in case they form background to visibility targets; coronas around terrestrial light sources; rainbows also in foreground of target: glittering of ice fogs increasing veiling light.

Section I-C

SKY BRIGHTNESS

Working group: Dr. E. O. Hulburt

The two NRL reports included herewith represent Dr. Hulburt's report to the Subcommittee.

Discussion

Dr. Hulburt reported on his plans for making sky brightness measurements this winter. He told of the plans to work at Point Barrow, Alaska, where measurements will be made of the night sky, including particularly, aurora borealis phenomena. Dr. Hulburt stated his belief that these measurements, together with measurements made by Dr. Currie at Saskatoon, Canada, would provide an adequate basis for description of night sky brightness in polar latitudes.

Dr. Hulburt also reported his plans to measure sky brightness at various altitudes up to the upper limit to which a B-29 can go. He stated that at the time of the meeting he expected the crew had completed their measurements and were on their way back to Washington.

After considerable discussion, the Subcommittee requested Dr. Blackwell to measure horizon sky brightness at Roscommon in the daytime. Measurements were to be made between the horizon and a point 1° above horizon. It was suggested that a series of points be taken between zero and 1° elevation above the horizon, utilizing as small a field of the photometer as possible.

It was agreed that the measurements should be made concomitantly with a measure of sky brightness at a point 5° above the horizon. These measurements should be made under different conditions of sun, altitude, and weather conditions.

Section II-D

ILLUMINATION

Working Group: Dr. D. E. MacDonald
Comdr. D.R.E. Brown

Commander Brown reported for the working group and exhibited the composite curves relating illumination on a horizontal plane to altitude of the sun. Dr. MacDonald and Commander Brown tabulated and analyzed the large number of readings previously collected by Commander Brown in various parts of the world. About the time the work was completed, the excellent summary of illumination data published by Jones in the Journal of the Optical Society of America became available. Commander Brown stated that the two sets of data exhibited general agreement with some systematic disagreement at low sun altitudes.

Dr. Hulburt suggested that the illumination group study Jones paper and recommend any additional measurements which seemed necessary.

Section II-A-I

SUMMARY OF DETECTION THRESHOLDS

Working Group: Dr. H. Richard Blackwell

Since the Subcommittee meeting in January, two graduate students at Michigan have devoted half their time to the summary of detection thresholds for uniform symmetrical targets presented upon uniform backgrounds. The library references have been fairly well completed back to 1930, but we have yet to collate the material. The Library of Congress bibliography has been going on full speed since late January, but they are by no means finished with the task. It is possible that a summary report can be combleted by the end of the summer, or in the early fall.

Section II-A-2

SURVEY OF DETECTION THRESHOLDS

Working Group: Dr. H. K. Hartline, Chairman
Dr. L. A. Riggs
Dr. G. M. Byram
Dr. E. S. Lamar
Dr. H. R. Blackwell

Dr. Hartline reported that he had circularized general comments on the various topics in this section to members of the working group. Extemporaneous replies were received from all the members except Dr. Byram, who had been away on a field project. Dr. Hartline reported that various members of the working group appeared willing to work up the material in these various areas. The task of making the summaries is, however, a very lengthy one, and reports are not expected for some months. Dr. Hartline has agreed to edit and summarize the various comments obtained from various members of the group.

One change in the listing of a topic in this area was agreed upon by all members of the group. Item "f" should be changed to read simply "duration".

Section II-A-3

FIELD APPLICATION

Working Group: Dr. S. Q. Duntley, Chairman
Dr. E. O. Hulburt
Dr. E. S. Lamar
Comdr. D.R.E. Brown
Dr. H. R. Blackwell

At the request of the Office of Naval Research, the working group agreed to consider as its first order of business field application of visual data as related to the problem of submarine detection. Accordingly, the group met in New York on March 5, 1948.

The group recommended that two immediate steps be taken to explore the problems of visual detection of submarines. Dr. Hulburt, as Chairman of the full Subcommittee on Visibility and Atmospheric Optics, was requested to forward the recommendations to ONR. The following letter was submitted to ONR on March 9.

9 March 1948

To: Captain C. W. Shilling
Chief, Medical Sciences Division
Office of Naval Research

Subj: Submarine Visual Research Project, Recommendation for.

Refs: (a) BuShips Conf. ltr. SS/S19-7(631-5815-330) dated 12 Sept. 1947.
(b) CNO End-1 of ref. (a) Op-34H2:tl (SC) S17-7 Serial 01064P34 dated 30 Oct. 1947.

1. Refs (a) and (b) requested an investigation to determine the detection ranges of submarines under various conditions. The request was referred to the Subcommittee on Visibility and Atmospheric Optics of the Army-Navy-NRC Vision Committee for comment, particularly on those aspects of the problem which are concerned with visibility. A working group of the Subcommittee met on March 4, 1948, and concluded that a portion of the problem might best be solved by basic scientific investigation and a portion by observations of operating submarines. The group outlined the following suggestions for the preliminary stages of the problem.

2. It is suggested that exploratory experiments on a small scale be made of the basic physical and visual factors of submarine detection. The experiments would consist of measurements of contrast ratios of small models at various depths under water to determine the effects of the light transmission of water, angle of view, state of surface, and illumination factors as the state of the sky and bearing and altitude of the sun.

3. It is suggested that existing information pertinent to the detection of submarines by the eye be made available for study, particularly any photographs and observational data obtained by Opdevfor in the Key West area. It is suggested that personnel from O.E.G. be requested to undertake this study.

4. It is suggested that the information obtained from the above two studies be made available in appropriate form to the working group of the Subcommittee on

the idea that the group examine the data obtained and recommend subsequent steps.

E. O. Hulburt, Chairman,
Subcommittee on Visibility and Atmospheric
Optics of the Army-Navy-NRC Vision Committee

In accordance with paragraph 2 of Dr. Hulburt's letter, and following special request from the working group, on April 20 Dr. Duntley submitted a proposal to ONR. The following excerpts indicate the nature of the proposal.

> Engineering methods can be applied to the visibility of submerged objects if adequate data can be secured concerning each physical factor. These factors include: (1) the quantity, quality, and spacial distribution of daylight; (2) the reflectance and shape of the submerged object; (3) the transmitting and scattering properties of sea water; (4) the refractive effects of the surface of the sea; (5) the contrast reduction by reflections in the surface of the sea; and (6) the contrast reduction by the atmosphere above the sea. Adequate, or nearly adequate, data appears to be available for items (1), (2), and (6). Much is known about item (3), but certain additional data appear necessary. The experimental technique for getting these data in usable form requires exploration, and this is one mission of the proposed exploratory investigation. Items (4) and (5) are almost completely unknown. The Florida data referred to in paragraph 2 is scarcely a beginning. The working committee is unwilling to attempt to recommend experimental procedures to be used by the Navy in full scale experiments at sea until items (4) and (5) have been explored. This exploration is the primary purpose of the proposed experiments.
>
> The working committee has asked Professor Duntley to propose a contract between M.I.T. and ONR under which the exploratory investigation can be conducted under his supervision during the summer of 1948 in convenient inland waters. It is proposed, therefore, to conduct these experiments at Professor Duntley's summer residence on Diamond Island, Lake Winnipesaukee, New Hampshire.
>
> Tentative plans for the exploratory experiments are as follows: An observing platform will be erected on the south shore of Diamond Island. This platform will extend out from the island and will permit an observer to look down thirty feet or more into water deep enough to obscure submerged objects. A variety of objects bearing gray-scale markings will be anchored at various depths and azimuths. Photometric observations will be made under a wide variety of lighting and weather conditions. Both photographic and visual photometry will be used. A study will be made of means for specifying items (4) and (5) in terms of the state of the sea and the state of the sky measured by parameters which can be easily evaluated on ship-board or estimated from meteorological data. The exploratory experiments will be conducted during the summer but the contract should provide for the reduction of the data and the preparation of suitable reports during the fall of 1948.

On May 1, word was received from ONR that the proposal submitted by Dr. Duntley had been approved. Additional steps will be taken by the working group when the results of Dr. Duntley's test are available, and when the data requested in paragraph 3 of Dr. Hulburt's letter are available.

Discussion:

Dr. Lamar reported that he has received orders which permit him to examine the Opdevfor data in submarine sightings obtained at Key West. He stated that he expects to accomplish this mission during the summer months.

Section II-B

RECOGNITION THRESHOLDS

Working Group: Dr. H. Richard Blackwell

We are apparently a very long way from having any clearly formulated notions about recognition thresholds. The only progress made to date has been a partial clarification of the most important issues which we can expect to attack in the Subcommittee work.

Dr. Blackwell had the good fortune to spend one day in conference with Dr. J. J. Gibson concerning this general topic. Dr. Gibson was formerly a major in the Air Corps and was in charge of the rather large scale activities of the Santa Ana Unit for Aircraft Recognition Training. Dr. Gibson is convinced that the problem of recognition training is not one which is susceptible to the kind of experimentation which has been conducted in visual detection. He believes that aircraft recognition depends largely upon establishment of spatial relations in the visual field. Dr. Gibson has recently completed a book on perception in which he has formulated definite plans of research which may be expected to yield information concerning the perceptual basis of spatial relations. Although specific suggestions cannot be made at this time, it would seem as though Dr. Gibson correctly states that we may not expect problems of recognition training and even of recognition thresholds for such complicated objects as aircraft to be determined with the kind of psychophysical methods used in the detection studies.

Dr. Blackwell also talked with Dr. Coleman who is very much interested in the influence of contrast upon recognition thresholds of a simple sort, such as resolution. Dr. Coleman's concern for this problem arises from his research activities in the direction of specifying optimum characteristics for optical devices. The same concern is shared by Dr. Duncan MacDonald. The most concrete step in this direction is the planning of a symposium on the topic of "Visual Thresholds; Their Relation to Optical Instrument Design", which is tentatively scheduled for Boston on July 1. Dr. Coleman is scheduled to present a paper on the general topic of contrast reduction in the atmosphere and in optical instruments. Dr. Blackwell is scheduled to deliver a paper concerning the effect of contrast reduction upon visual thresholds. We are all familiar with the experimental data concerning the influence of contrast upon visual detection. Dr. Blackwell has recently collected some additional data which are pertinent to the effect of contrast reduction upon resolution thresholds.

It is believed that the general area will be much clarified by the symposium in July. Perhaps more concrete suggestions will be possible at the next meeting of the Subcommittee.

An additional topic coming under the general heading of recognition is the problem of chromatic thresholds for point sources. There has been some scattered work in this field, but the work is certainly out-dated by the recent

findings of the British workers relative to the spectral sensitivity of the fovea for objects of small subtense. If this question is of great importance, the Subcommittee might wish to discuss the possibility of collecting data of this sort.

DISCUSSION:

The group was in general agreement that recognition of complex objects might have to be studied in a different manner from detection of simple objects. There was also, however, a strong conviction that recognition research should make full use of detection data as the lower limit of discriminability.

It was agreed that, in the Subcommittee's work, this section would be concerned first with simple visual discriminations in which some aspect of the stimulus had to be recognized, such as resolution. Perhaps the Subcommittee will be able to extend the work of collating information to recognition of complex objects, but to do so, it will be necessary to expand the membership to include psychologists interested in perception.

GENERAL DISCUSSION:

Mr. Middleton called the group's attention to the following book:

F. Lohle. Sichtbeobachtungen vom meteorologischen Standpunkt. Berlin, Verlag von Julius Springer, 1941

Mr. Middleton stated that so far as he knew the copy of the book which he has managed to get out of Germany may be the only one in this hemisphere. He stated his willingness to lend the book for photostating if members of the Subcommittee thought it profitable.

After considerable discussion of the various field projects being conducted, the following recommendation was approved:

IT WAS THE CONSENSUS OF OPINION OF THE SUBCOMMITTEE ON VISIBILITY AND ATMOSPHERIC OPTICS THAT APPROPRIATE STEPS BE TAKEN TO EXPAND DR. COLEMAN'S PROJECT THIS SUMMER TO INSURE ADEQUATE MEASUREMENTS SO THAT DEFINITE RECOMMENDATIONS CAN BE MADE IN THE FALL CONCERNING DESIRABLE FUTURE MEASUREMENTS OF ATMOSPHERIC ATTENTUATION.

ABSTRACT OF DR. HULBURT'S REPORT OF THE SUBCOMMITTEE
ON VISIBILITY AND ATMOSPHERIC OPTICS

The Subcommittee on Visibility and Atmospheric Optics met May 26, at New London, Connecticut. The meeting was devoted to a discussion of technical reports which are being prepared by the various working groups of the Subcommittee. The general outline of the areas covered by technical reports and the working groups which have been appointed, can be found in the Minutes of the first meeting of the Subcommittee on Visibility and Atmospheric Optics (January 26-27, Washington D.C.).

Members of the various working groups reported their progress. Particular attention was given to current research being conducted in several of the areas in this field. The general plan of action involves preparation of a technical report in each of the areas selected. The technical reports will be made available to the Armed Services. The group will not attempt to prepare manuals or other information involving specific applications of the more general data.

The working group on atmospheric attenuation reports considerable activity, including several research programs going on at the present time. Dr. Coleman will continue to measure the relation between transmission of light through the atmosphere and distance in a new experimental setup at the University of Texas. In connection with vision tests being conducted by Dr. Blackwell, four visibility meters will receive testing this summer at Roscommon, Michigan. Three of the meters have been made available through Mr. Middleton from scientific workers in England. The fourth meter was constructed by Dr. S. Q. Duntley. It is hoped that the next year will see considerable information gained in this area.

The working group on atmospheric refractive phenomena has produced a very comprehensive outline of topics, and Dr. Neuberger and Mr. Harrison are preparing a technical report. Experimental tests of scintillation, or shimmer, are being conducted in Akron, Ohio, under the direct supervision of Dr. Blackwell.

Technical reports on brightness of the sky, day and night, have been prepared by Dr. Hulburt, and are already published. Additional research is being planned by Dr. Hulburt which will yield data in polar latitudes, with particular reference to the aurora borealis phenomenon. In addition, a program of measurements of sky brightness as a function of altitude is being conducted at the present time, utilizing a B-29 aircraft. Presumably, at the time of reporting, these data are complete. Dr. Neuberger has proposed an experimental program of measurements of polarization of the sky in connection with turbulence in the air. This general area is receiving considerable attention, and certain aspects of it can be said to be sufficiently well understood at present.

The problem of illumination is also quite well understood. A technical report will be worked up by Dr. Macdonald and Commander Brown. Commander Brown and Dr. Macdonald have prepared an illumination computer which will be demonstrated to you this morning.

The remainder of the work of the Subcommittee is concerned with visual response. The first task in this area is a compilation of existing data relevant to the case of uniform targets seen under uniform conditions.

Dr. Blackwell and his students are working on this material for presentation next fall. The experimental data in this area are being extended by field tests conducted at Roscommon, Michigan, by Dr. Blackwell.

Dr. Hartline and his working group are attempting to outline the general status of additional visual problems which are of Service importance. These general areas include problems which are basically theoretical; many of them are very poorly understood at the present time. The working group will attempt to collect the scattered data and to indicate profitable lines of further research. Most of this material cannot be said to be well understood, nor is it at all near completion.

The working group under Dr. Duntley is concerned with certain field conditions which complicate the application of visual data in military situations. At present, this working group is concerned with the problem of submarine sightings from the air. Two specific activities are being carried on. Dr. Duntley is planning to explore factors influencing the thresholds of submerged objects. Dr. Lamar is going to examine operational data obtained on actual submarine maneuvers in order to estimate some of the various factors involved. Another problem which has come to the working group concerns the influence of vibration, or unsteadiness of instruments, upon visual thresholds.

The final section concerns certain aspects of resolution, working toward the recognition problem from the threshold side. This area is only beginning to take shape in the minds of the Subcommittee members.

We feel that the Subcommittee has been particularly active during the past months and trust that significant progress can be made within the next year.

Abstract: FIELD TESTS OF VISIBILITY OVER WATER

S. Q. Duntley

Massachusetts Institute of Technology

Buships and CNO have requested information concerning the ranges at which submerged submarines are visually detectible under various conditions; and the working group concerned with visibility under field conditions has considered the possibility of applying engineering methods of visibility prediction to this problem.

Engineering methods can be applied to the visibility of submerged objects if adequate data can be secured concerning each physical factor. These factors include: (1) the quantity, quality, and spacial distribution of daylight; (2) the reflectance and shape of the submerged object; (3) the transmitting and scattering properties of sea water; (4) the refractive effects of the surface of the sea; (5) the contrast reduction by reflections in the surface of the sea; and (6) the contrast reduction by the atmosphere above the sea. Adequate, or nearly adequate, data appears to be available for items.(1), (2), and (6). Much is known about item (3), but certain additional data appear necessary. The experimental technique for getting these data in usable form requires exploration. Items (4) and (5) are almost completely unknown. The working committee is unwilling to attempt to recommend experimental procedures to be used by the Navy in full scale experiments at sea until items (4) and (5) have been explored.

The working committee asked me to propose a contract between M.I.T. and ONR under which the exploratory investigation can be conducted under my supervision during the summer of 1948 in convenient inland waters. M.I.T. proposed, therefore, to conduct these experiments at my summer residence on Diamond Island, Lake Winnipesaukee, New Hampshire, and a contract for this research was subsequently negotiated.

Tentative plans for the exploratory experiments are as follows: An observing platform will be erected on the south shore of Diamond Island. This platform will extend out from the island and will permit an observer to look down thirty feet or more into deep water. A variety of objects bearing grayscale markings will be anchored at various depths and azimuths. Photometric observations will be made under a wide variety of lighting and weather conditions. Both photographic and visual photometry will be used. A study will be made of means for specifying items (4) and (5) in terms of the state of the sea and the state of the sky measured by parameters which can be evaluated on ship-board or estimated from meteorological data.

No final data for operational use are expected to result from the exploratory experiments, but it is hoped that this study of the problem will enable the working group to recommend specific experiments at sea which can yield the information needed to apply engineering methods to the prediction visibility of the submerged submarines.

Discussion

Dr. Zigler asked Dr. Duntley why he didn't increase the elevation above the water for viewing submerged objects.

Dr. Duntley replied that it might be necessary to increase the elevation, but that because of the cost, he hoped to be able to get most of the answers from the contemplated height.

Dr. Zigler asked why Dr. Duntley didn't consider using an airplane and working at a height of 1,000 feet.

Dr. Duntley replied that two factors were involved. In the first place, increasing the altitude increases the whole scale of operations, including target size, which increases the cost of the operation. In addition, the amount of observing time becomes very much reduced because of weather conditions. The Tiffany group attempted this sort of measurement in Florida in 1944, and that experience has persuaded Dr. Duntley to work from a platform.

Captain Willmon asked whether Dr. Duntley had considered working from a blimp.

Dr. Duntley replied that this possibility had been considered. As a matter of fact, operations of this sort are being conducted; Dr. Lamar plans to make a study of the data which had been obtained in this way. The experiments Dr. Duntley plans to conduct are intended to explore the problem with particular reference to the kind of instrumentation which will be necessary if larger scale trials are conducted subsequently.

Dr. Gibson pointed out the importance of the exact pattern of wave forms on the surface, which are of course determined by weather conditions.

Dr. Duntley agreed that the patterning of wave form is a very important variable, and stated that, eventually, measurements of this sort would have to be made at sea. He stated his belief that preliminary tests could be profitably conducted in the most convenient way in order to obtain an estimate of the kind of instrumentation and the procedures which should be developed for the more elaborate testing.

Dr. O'Brien asked whether Dr. Duntley cared to describe the kind of photometric measurement which he was planning to make of the various submerged objects used in the tests.

Dr. Duntley replied that various kinds of measurements would be explored. He stated that, probably, photographic photometry would be used in order to obtain measurementsof the conditions present.

Dr. Wolf suggested utilizing visual photometers as well as photography.

Dr. Duntley replied that these photometers, and photoelectric instruments as well, would undoubtedly be utilized before the tests were over.

FIELD TESTS OF VISIBILITY OVER LAND

H. Richard Blackwell
University of Michigan

During the war years, the general problem of specifying the range at which visual detection is possible received considerable attention. In particular, the Camouflage Section of NDRC conducted both laboratory tests of visual detection and exploratory tests of atmospheric optics. The findings were reported before the Vision Committee in November, 1945, by Professor Hardy, Professor Duntley, and the speaker. At that time, it was apparent that many supplementary investigations were necessary before adequate predictions of visual range could be made. The existing data are most adequate in considerations of slow moving targets, seen against the sky or located in the sky.

Late in 1946, a working group consisting of Professors Duntley, Coleman, Neuberger, and the speaker, began making coordinated plans for exploring the adequacy of the existing data. It was decided that, owing to greater difficulties encountered over water, the initial experiments would be conducted over land. Two programs were agreed upon: The Penn State tests of atmospheric optics were planned and preliminary results reported by Professor Coleman in January, 1948. Continuation of Professor Coleman's program at the University of Texas has been arranged through contract with ONR. In addition, the 1947 Roscommon tests of visual detection in the field were planned and conducted by the University of Michigan, a report of which was made to the Vision Committee in October, 1947. It is my purpose here to outline the additional field tests of visual detection planned for this summer by the Vision Research Laboratory at the University of Michigan.

At the outset, it is necessary to acknowledge the valuable assistance given by Professors Neuberger, Duntley, and Coleman, and also the general assistance of other members of the Vision Committee's Subcommittee on Visibility and Atmospheric Optics. As we shall see, many phases of the program would not have been possible without their cooperation.

Application of laboratory visual detection thresholds to the field situation involves, first of all, consideration of atmospheric effects. In addition, it is necessary to evaluate differences in attention and motivation of the observers and in the perception of distance and other field conditions. In order to simplify comparisons between the laboratory and the field situation, the differences in attention and motivation of the observers should be eliminated as much as possible. This has been accomplished in our field tests by exactly duplicating the laboratory method of psychophysics. The principal variables we believe we are encountering in our comparison between laboratory and field data are, therefore, (1) atmospheric effects, and (2) psychological differences in the appearance of objects in the field.

The tests of visual detection in the field this summer will be conducted at the location previously employed in our exploratory studies. The principal facilities include 5 fire lookout towers, located in a wooded region of northern Michigan. One centrally located tower serves as the observation post. Target towers are visible as silhouettes against the horizon sky at distances of 6, 10, 20, and 30 miles.

Each target tower is equipped with an orientation light, which will insure that the observers are adequately fixated during all the experimental runs. The orientation light consists of a 60" searchlight reflector and lamp assembly, specially modified to permit remote control of the onset and offset of the light by means of a shutter. A green filter gives the orientation light a qualitative difference from the stimulus objects. The orientation light can be reduced in intensity so that it will be just barely visible under the various conditions tested.

In addition, each tower is equipped with a multi-purpose target. There is provision for a reflection-type target, of variable reflectance, to be used when the sun is shining on the tower. This target can be adjusted to match the sky and then rapidly adjusted to the appropriate reflectance to render it visible to any desired extent. In addition, there is provision for a self-luminous target for use at night or under cloudy or overcast conditions in the daytime. The self-luminous target consists of a diffusing screen, 6 feet square, illuminated uniformly to a brightness of 2450 footlamberts by some 200 fluorescent bars mounted in an integrating box behind the screen. By means of absorbing screens and louvres, it is possible to obtain continuous reduction in the brightness of the self-luminous target from 2450 to 1×10^{-2} foot lamberts without disturbing the candlepower distribution. The self-luminous target can also be adjusted to match the sky and then abruptly readjusted to any desired brightness contrast with the sky.

Power for the light sources will be provided by gasoline-driven motor generators located at the base of each of the towers. The multi-purpose targets will be mounted at the 50 foot levels, resting on special platforms constructed last summer. The orientation lights will be located on supplementary platforms located near the tops of the towers. The towers will be repainted a suitable gray so that they will provide the best possible match with the sky, under the conditions of use.

At each tower, a recording 35 mm. still camera will be mounted on a boom directly in front of the target, so that a record can be made of the brightness contrast of the target and the sky at any moment. Gray scales will be mounted on the tower for calibration purposes.

Communications will be maintained between the towers by means of FM radio transmitters and receivers. In addition, Navy-loaned jeeps will be available for transportation among the towers and the base of operations.

In the daytime, the clarity of the atmosphere cannot be determined with sufficient precision in advance to permit selection of the most appropriate target tower. Therefore, during visual threshold runs, in the morning, the 6 and 10 mile target towers will be manned; in the afternoon, the 20 and 30 mile target towers will be manned; while at night, any one of the four towers may be used.

The experimental procedure will involve successive presentations of various known target contrast values. Two observers will record their success in detecting the presence of the target by guessing in which of four time intervals it occurred. The contrast of the target reaching the observer's eye will be recorded on 35 mm. movie film by a special optical system. The telescopic objective to be used was loaned to the project by Dr. Duncan MacDonald, Optical Research Laboratories, Boston University. Simultaneous re-

cord will be made of the contrast of the target at zero distance, by means of the still camera mounted on the boom. These measurements will not only permit us to determine the detection threshold of the eye, but also the degree of contrast reduction introduced by the atmosphere at the moment of the test.

Analysis of our data consists, first, in merely comparing detection thresholds for a series of runs conducted on different days, under different atmospheric conditions, one measure of which is the contrast reduction value. Other descriptive measures will be available. The Weather Bureau has kindly made available three recording hygrothermographs to be located on the ground, and at 50 and 100 feet above the ground. With calibrating psychometric readings, these instruments will permit us to compare temperature and humidity values at the three heights. In addition, Professor Neuberger has developed special counters which will permit us to determine the concentration of condensation nuclei at the time of our experimental runs. We are also planning to measure sky brightness in absolute terms, and incidental illumination.

For those days in which atmospheric boil is absent, we do not expect there to be reliable differences in detection threshold at the eye, regardless of the atmospheric conditions. If this prediction can be verified, then we are apparently justified in ignoring the rather large differences in appearance of objects concommitant with the differences in atmospheric conditions.

When atmospheric boil is present, the rate and magnitude of the fluctuations will be determined from the motion picture records. If, as we expect, the fluctuations are of large magnitude and rate sufficiently slow so that the eye does not integrate them, we will need to be able to determine the effective intensity of similar slowly fluctuating light in the laboratory. Such determinations will permit us to separate out the atmospheric effects, and thus, to evaluate the psychological differences between laboratory and field.

The second main analysis represents an attempt to predict visual detection thresholds from the variables: target size and contrast, sky brightness, and a measure of atmospheric transmissivity. Target size is of course known. Contrast of the target at zero distance is recorded. Sky brightness will be measured with a Macbeth Illuminometer specially fitted with a telescopic system. Considerable effort has been made by members of the Subcommittee to provide simple practical devices for measuring atmospheric transmissivity. Professor Duntley has constructed a polar nephelometer, based on the general design suggested by Britains's Waldrum during the war. This device measures transmissivity for a collimated beam, which may well prove to be an inadequate index of contrast reduction for a diffusely emitting source. Mr. Middleton, President of the Commission on Atmospheric Optics of the International Meteorological Organization, has managed to borrow three experimental instruments from England--the Brewer and Beutell instrument, the disappearance range gauge, and the "Loofah". We should be able to determine the degree of precision in predicting visual detection ranges by use of these instruments.

Because of the rather unusual equipment available for the field tests, members of the Subcommittee have suggested that certain measurements of atmospheric optics be incorporated in the tests. It will be possible to measure Koschmieder's law of the attenuation of target contrast as a function of distance, for a few very clear days. In particular, the high original brightness of the self-luminous targets will make it possible to test Professor Duntley's prediction of a breakdown of Koschmieder's formula for high target contrasts.

In addition, it will be possible to test Allard's law for the attenuation of intensity as a function of distance at night. It should be possible to determine whether failure of Allard's law is due to a failure of the inverse square law, or to a variation in the threshold illumination required by the eye when the target distance is varied.

In addition, it will be possible to analyze the physical data to ascertain the correlation between various meteorological conditions and atmospheric transmissivity and atmospheric boil.

The Vision Research Laboratory has undertaken a second project concerned with the relation of standard meteorological quantities to the degree of atmospheric boil. The project is merely an appendage to a large-scale meteorological project being conducted by the Guggenheim Airship Institute of Akron, Ohio. Members of the Subcommittee have participated in the planning of the project to a quite considerable degree. We are planning to record the fluctuations in intensity of a series of light sources, viewed through a mile of atmosphere by a 16 mm. recording system. The relation between degree of boil and light source size will be determined under a number of critical meteorological conditions. In addition, boil will be recorded daily over a several month period to obtain frequency distribution data. The Guggenheim project will measure temperature, humidity, and velocity at a number of heights above the ground up to 350 feet.

Discussion

Dr. Hulburt asked how far above the horizon line the targets were located.

Dr. Blackwell replied that the angular separation between target and horizon varied from 1 to 4 minutes of arc, for the various towers.

Dr. Hulburt asked if Mr. Middleton would care to comment on the three visibility meters which are being tested in conjunction with the Roscommon tests.

Mr. Middleton stated that the three meters are, basically, of two classes. The first of these is the disappearance range guage, which is a highly developed model of a meter originated by Shallenberger and Little. The meter is placed in front of a telescope. Two images of the horizon are presented and the instrument adjusted until one of the images disappears. Knowing the acutal distance of the horizon, the visual range can be computed from a special scale.

The other two instruments, the Brewer and Beutell instrument and the "Loofah" are of a fundamentally different principle, designed for use at night. In effect, these instruments illuminate a column of air and measure the scattering coefficient at various angles from the incidence of the light rays. Experiments have indicated the utility of these measurements in predicting the range at which objects will be seen at night.

Mr. Middleton commented that the instruments have not been given a good test and that the Roscommon tests should provide a very nice way of checking the devices. The instruments may work and they may not.

Abstract of: COMMANDER BROWN'S REMARKS CONCERNING HIS

ILLUMINATION COMPUTER

There is a substantial amount of literature relevant to the illumination on a horizontal surface as a function of solar altitude for clear and cloudy days. During the last few years, Commander Brown and his colleagues obtained considerable data in different parts of the world. With the assistance of Dr. Duncan MacDonald of Boston University, the data were recently analyzed and average curves computed. In order to make illumination data available to the average individual, who does not have access to hydrographic tables, Commander Brown prepared an illumination computer which presents the illumination on cloudy and clear days as a function of time of day, day of year, and latitude.

Commander Brown offered to furnish members of the Committee with illumination computers upon request.

SIGNAL LIGHT SPECIFICATIONS

Dean Farnsworth, Lt. Comdr., H(S) USNR

This paper gives some background on the conflicting requirements implicit in the design of systems of color signals. It involves the questions of standardization of color signals, and of the selection of personnel for color vision.

The International Commission on Illumination, which meets in Paris this July, will discuss international standardization of signal lights. The diversity of present standards is suggested by the few boundaries which are plotted in Figure 1. Only eleven of the specifications are shown out of the dozens with which the Allies operated. It is evident that certain similar standards have been adopted for diverse uses (for instance, see numbers 2 and 7), which suggests that each separate service may not require a different set of lights. On the other hand, diverse standards have been adopted for the same uses (for instance, see numbers 2 and 3), which suggests that one or the other service, or both, may not have arrived at the most efficient standard.

There is no conceivable reason why British and American Railways need different signal lights or why road traffic lights in Canada should be different from road traffic lights in America; nor why a manufacturer should be expected to meet a new specification requiring new standards each time he receives an order from a different service in America - Air, Army, Navy, Marine, or Railway. J.G. Holmes stated the need for simplification in the Foreword to the new British Specifications,

> "The existence of several slightly differing specifications was felt to be unnecessary and to lead to manufacturing difficulties which result in delays in supplying the requirements of different users. The problems involved in the recognition of signal colors are largely common to all users, and the differences which occur in the various specifications have, in many instances, no fundamental justification, having arisen chiefly from the independent experience of the different users."

It is one thing to see the clear need for a policy and another to achieve agreement of that policy in practice. On the request of the Bureau of Medicine and Surgery, this laboratory developed a set of new specifications[1] which it was felt would simplify the problem by dividing it into two classes so that we would have not twenty but two sets of specifications for most purposes. The boundaries of these specifications are shown in Figure 2. They are based upon the two categories according to use. The first set covers the need for lights which are visible from the greatest possible distance while still retaining some color quality, and these may be designated as high visibility limits: obstruction lights on tall buildings and wireless towers or airport runway lights come under this heading. On the other hand, there are lights which depend for their effectiveness on their recognition as red or green or white or blue, such as most stop-and-go lights and, more specifically in the Submarine Service, the lights of the "Christmas Tree" showing valves open and valves closed - a situation in which all lights must be green before the ship can be submerged and in which there is likely to be only one mistake in the lifetime of a ship.

A rational solution to this problem of standardization must effect the

mutual reconciliation of the following requirements:

(1) They must not impose undue manufacturing difficulties.

(2) Specified boundaries should follow psychological thresholds of recognition.

(3) Transmission should be high for brightness, but purity should be high for distinguishability.

(4) The number of signal lights that will be employed in a given system modifies the position of the individual colors.

(5) Distinction must be made between lights which will be employed for high visibility as opposed to those whose color must be recognized unerringly.

(6) Filter specifications must be adjusted to the color temperature of the illuminant.

(7) Certain signal lights will be used with reduced voltages which change the color of the signal.

(8) Colors must be distinguishable at small subtense as well as at high intensity.

(9) Colors should be chosen to be as distinguishable as possible by partial color blinds.

A few examples will illustrate some of the difficult compromises that must be made between the above requirements. With reference to (1) and (2), we have recommended a boundary in the near white region which is based upon the psychophysical limits of equally perceptible differences of greens from whites. This is based upon the work of Holmes[2] and Hill[3], which is summarized in Figure 2 by the curved boundaries 1-6. Psychologists think this is very reasonable, but the manufacturers think it most unreasonable because most green glasses when polished down to successively thinner sheets follow another kind of a curve. Formerly, both a thick and a thin moulding of the same melt would be acceptable, whereas the New London specifications would undoubtedly cause more rejections. The psychologists' reply would be that the rejected glass would be as dangerously confusing as some not permitted under present specifications.

Another difference of opinion concerns the upper boundaries of the green region, involving requirements (3) and (9). Take the case of green lights which are used for identification in aviation. The argument runs that any light in this region - be it ever so yellow - is a satisfactory green. The reply to this is that it is highly confusable with white and red by color blinds and if it has any function as a green at all no such light should be tolerated. The British include this yellow-green region for another reason. It is claimed simply that, since no glasses are at present manufactured which are near the spectral locus in this region, it does not matter if the specifications run up that high. The obvious reply is that, if glasses don't exist in that region, it is safer not to permit specifications which extend that far in that region.

A third and related question concerns the admissibility of an orange-red

extending as far up as 610 mu, which concerns requirements (3) and (8). It is well-known that a bright red light appears more yellowish than a dimmer red of the same spectral transmission, and there are engineers who have claimed that brilliant orange-reds have appeared to them as an amber signal. These are exceptional cases and lead to a suspicion that the engineers were partially color defective.

Another difficulty is based upon the peculiarities of color vision itself (item 8). The occasions when the colors of signal lights are important are when they are seen at small subtense, that is, as points of lights at a distance. Under these circumstances, all yellow-blue perception is washed out and only greenness, whiteness (or yellowishness) and redness remains. Unfortunately, this is exactly the series of colors which is diminished for color blinds who, however, retain yellow-blue perception. Compromise is possible by the use of reds with some yellow transmission and greens with high blue transmission. This color system can be recognized, except at threshold, by both normals and by partial color defectives.

The emphasis upon the need for considering borderline color blind people (item 9) is based upon several facts which we cannot ignore. One is that they are in the armed forces already - tens of thousands of them. A second fact is that borderline color deficiency is due to many psychophysical factors, which are hard to measure with certainty, and, consequently, borderlines will probably continue to be accepted. A third fact is that in the vast majority of situations they make as accurate use of color signals as do normals. A fourth fact is that the use of the border-line color deficients effectively increases the man-power reserve by about 2%, and would delay the sudden dropping of vision standards which occurs whenever a service begins scraping the bottom of the barrel. These four facts indicate that we should consider the borderline color deficient when preparing specifications for code colors.

The specifications proposed for high visibility green (limits A) in Figure 2 extend as far as the new British boundaries in the blue region, but are much more restrictive in the yellow-green region, the latter being identical with the U.S. Railway limits. Glasses within boundary B should be used not only where high recognition is necessary, but with lamps which are supplied with variable voltage. Similar limits, A and B, are proposed for red signals which are designed to exclude glasses with any short-wave transmission.

It is believed that the proposed limits strike a good compromise between the nine fundamental requirements and that one of the two sets of limits will be suitable for any color signal system. When new needs arise, it is strongly recommended that one of these sets of limits be considered in preference to writing new, similar, but different standards. We should be conscious of the reasons for exact specification of our signals and be alert in maintaining those standards.

References

1 Dean Farnsworth, "Proposed Specifications of Red and Green Navy Signal Lights," BuMed Project No. X-265 (Av-153-c), Color Vision Report No. 16, U. S. Naval Submarine Base, New London.

2 J. G. Holmes, "The Recognition of Colored Light Signals," Trans. Ill. Engr. Soc. (British) 6, 71-97, (1941).

3 N. E.G. Hill, "The Recognition of Colored Light Signals Which are Near the Limit of Visibility," Proc. Phys. Soc. (London) 59, part 4, 560-573, (1947) and "The Measurement of the Chromatic and Archromatic Thresholds of Colored Point Sources Against a White Background," 59, 574-585 (1947).

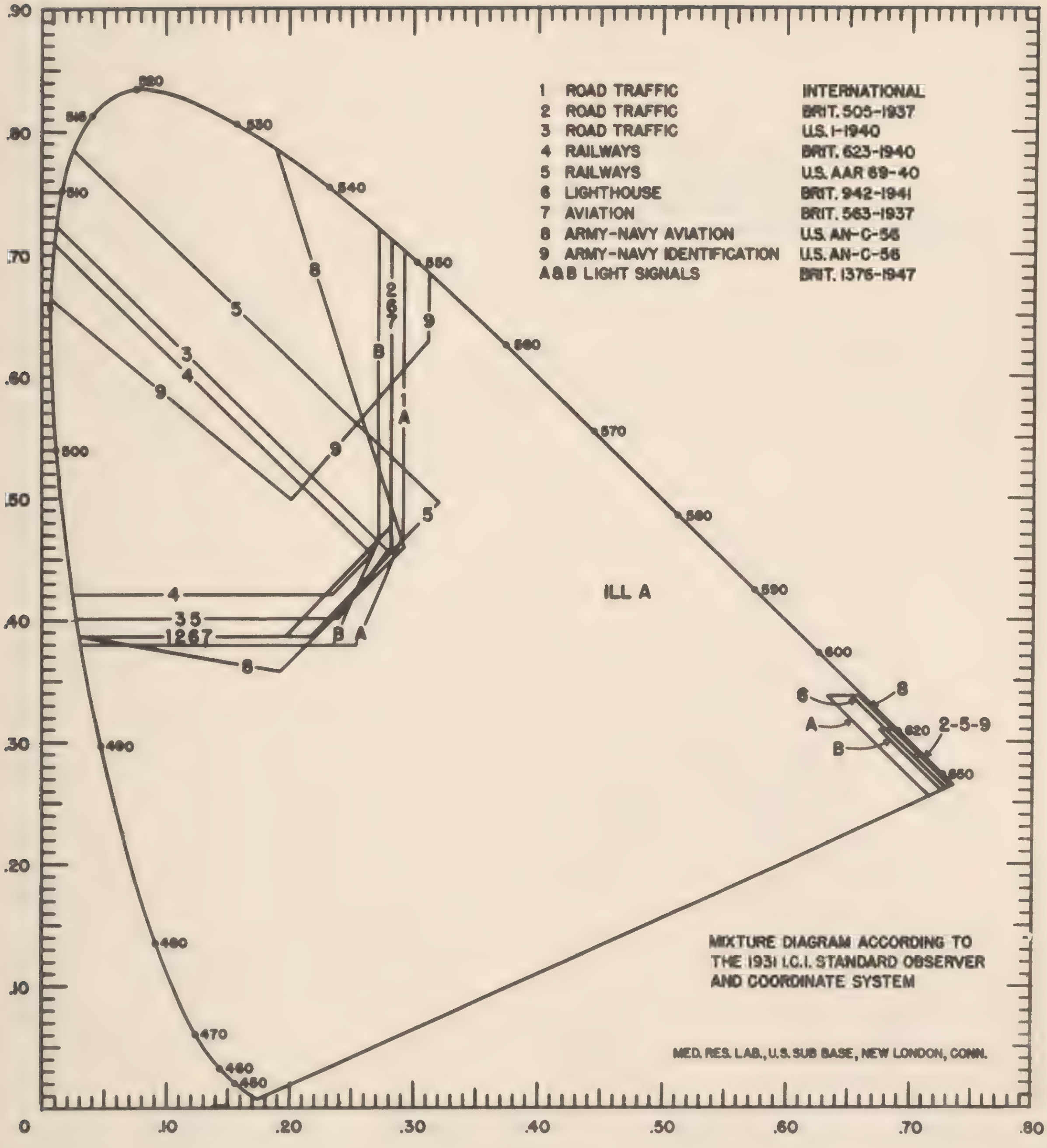
Discussion

Mr. Middleton commented that Commander Farnsworth is following very good historical precedent in his proposal for specification of signal light systems. Mr. Middleton agreed that there are far too many specifications, but stated that a great deal of progress has been made in this country and in Europe in standardizing them, and that perhaps the situation was not quite as bad as it might appear at first glance.

Commander Farnsworth replied that progress has been made in specification, but that there are, basically, too many separate sets of standards, each of which differs from the others.

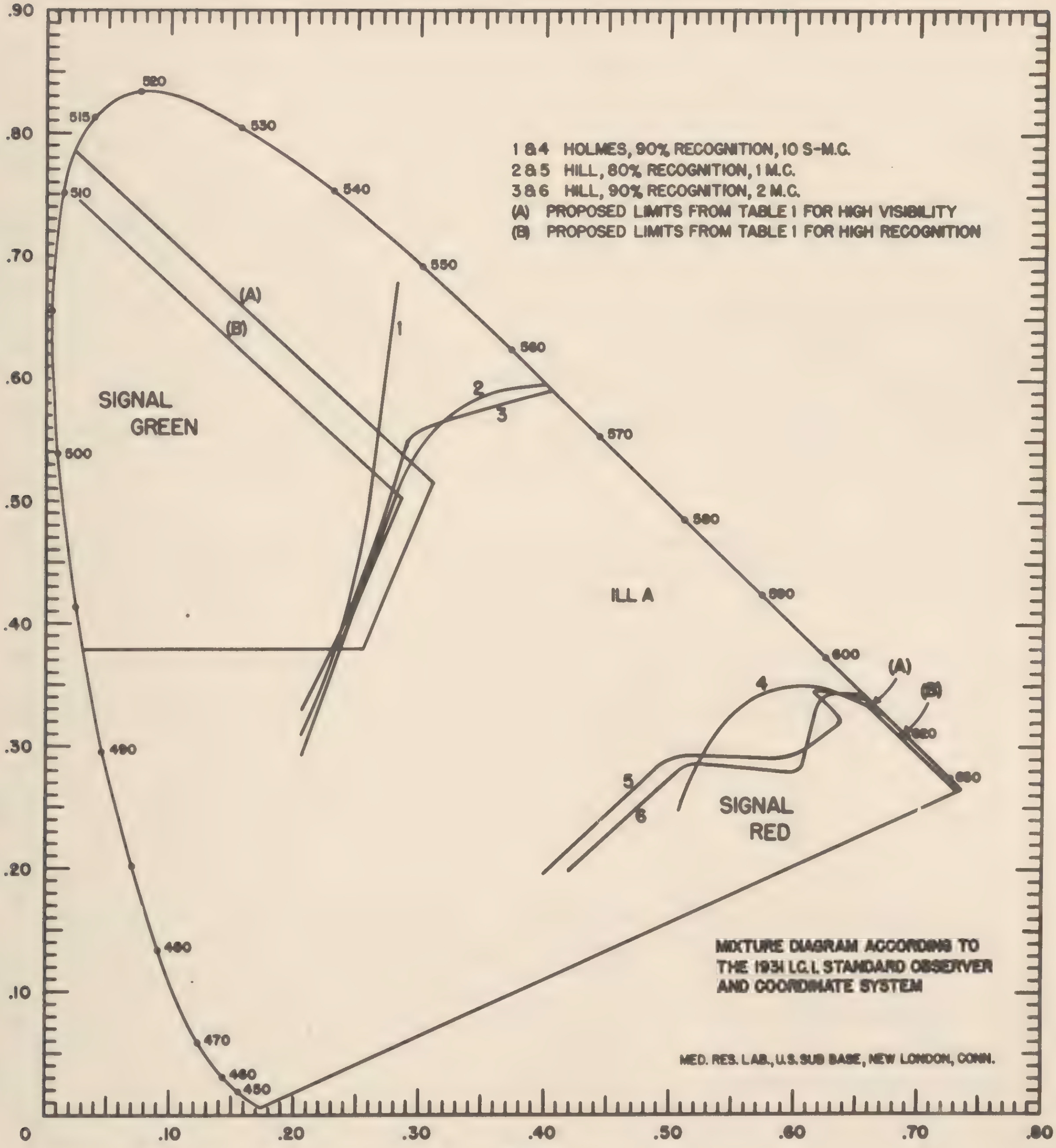
Dr. Sloan questioned whether the ICI system is meaningful in the specification of point sources, since we are well aware that the spectral sensitivity of small portions of the fovea differs from the ICI curves.

Mr. Middleton agreed with Dr. Sloan, but emphasized the near impossibility of obtaining color mixture data with point sources.



1 ROAD TRAFFIC INTERNATIONAL
2 ROAD TRAFFIC BRIT. 505-1937
3 ROAD TRAFFIC U.S. I-1940
4 RAILWAYS BRIT. 623-1940
5 RAILWAYS U.S. AAR 69-40
6 LIGHTHOUSE BRIT. 942-1941
7 AVIATION BRIT. 563-1937
8 ARMY-NAVY AVIATION U.S. AN-C-56
9 ARMY-NAVY IDENTIFICATION U.S. AN-C-56
A & B LIGHT SIGNALS BRIT. 1376-1947
ILL A
MIXTURE DIAGRAM ACCORDING TO
THE 1931 I.C.I. STANDARD OBSERVER
AND COORDINATE SYSTEM
MED. RES. LAB., U.S. SUB BASE, NEW LONDON, CONN.

FIGURE I



1 & 4 HOLMES, 90% RECOGNITION, 10 S-M.C.
2 & 5 HILL, 80% RECOGNITION, 1 M.C.
3 & 6 HILL, 90% RECOGNITION, 2 M.C.
(A) PROPOSED LIMITS FROM TABLE 1 FOR HIGH VISIBILITY
(B) PROPOSED LIMITS FROM TABLE 1 FOR HIGH RECOGNITION
SIGNAL GREEN
ILL A
SIGNAL RED
MIXTURE DIAGRAM ACCORDING TO THE 1931 I.C.I. STANDARD OBSERVER AND COORDINATE SYSTEM
MED. RES. LAB., U.S. SUB BASE, NEW LONDON, CONN.

FIGURE 2

RELATIONS BETWEEN VISION AND AUDITION

J. Donald Harris

Medical Research Laboratory
U.S. Naval Submarine Base
New London, Conn.

Introduction

At first glance, it might seem that the eyes and the ears have nothing in common except that both are placed on the head, thus serving along with the nose to alleviate the monotony of an otherwise unrelieved sphere--or, in some cases, cube. Differences are, in fact, more numerous than similarities. For example, the two organs handle totally different regions of the spectrum, and derive from different primitive germ layers. Furthermore, acoustics developed with the elegant formulations of Lord Rayleigh in the last century, and presents a reliable and coherent picture, while visual science, with its standard candle, still depends upon the consistency of the physiology of the whale. Out of consideration for this group, I will not list all the inconsistent whales I have known.

However, if we may for the sake of argument grant both sciences equal status, we may do much more than cite curious cases of synaesthesia, such as the little girl who hears colors, or the congressman who sees red when he hears a principle.

We may hope to pass from a brief review of sensory interaction, to a consideration of sensory and neural activities common to both sight and hearing, and finally, perhaps, to an examination of general principles of psychological organization and theory of the whole sensorium.

Intersensory facilitation.

Theoretical.

If somewhere in the nervous system the visual and auditory systems do interconnect, it makes neurological sense that the one sense can influence the other. Indeed, for many lower forms, unimodality cues are insufficient to arouse a reflex (Pike and Coombs); somewhere, the afferent stimuli must summate. For example, in the sea anemone, a combination of contact plus chemical stimuli is needed for food-taking (Nagel). Yerkes (1905b) showed that a frog would not jump to a small red card unless a noise was sounded a moment before. This type of facilitation can readily be quantified (Yerkes, 1905a).

There is little doubt that this intersensory facilitation is related directly to the somewhat simpler concept of summation, which we must now examine if we wish to understand the connection between sight and hearing.

Helmholtz first described the summation of subliminal stimuli in muscle; this was abundantly confirmed for lower organisms by Richet (1879a, 1879b), Peotrowski, Locke, Jennings, Arey and Crozier, and many others, and extended to the intact heart by Basch. The concept of summation was extended to nerve by Gotch and Burch, working with two successive shocks. In a classic paper on summation, Adrian and Lucas inferred two types of summation: first, of the

local excitatory process (later proved by Katz), and second, at the neuromyal junction. This latter facilitation was first observed by Setschenow for the spinal cord, and by Richet (1877) for the cerebrum. Forbes was the first to suggest that reflex summation was not due to a second response from the same motoneurones, but rather due to other motoneurones affected by a central summation of the effects of two stimuli. Here we approach a possible explanation of intersensory facilitation.

Eccles and Granit distinguished between "concurrent" and "sustained" facilitation. They found that after the facilitating stimulus has been terminated, it may still take 5" for facilitation to subside. They felt that this long time, a manifestation of subliminal after-discharge, obviated the 'delay path' explanation of Forbes. It remained for Lorente de Nó (1932) to suggest closed, self-reexciting neurone chains based on his laws of plurality and reciprocity (1933). Eccles has further detailed the picture of how a single preganglionic fibre can, by what he terms a 'detonator' action because of its speed, cause a brief excitatory state in the region of the synapse. This excitatory state spreads rapidly, but decrementally, through the cell body of the neurone; two or more of these detonator actions, by temporal or spatial summation, or both, are necessary to bring the excitatory state of the postganglionic fibre to threshold.

We thus have a well-grounded theory to explain the "bahnung" of Exner, the dynamogenesis of Féré of the last century, and most of the phenomena today known as summation, facilitation, subliminal fringe, occlusion, after-discharge, and the like.

Now let us examine whether one or more areas do exist in the human nervous system, within which neural interactions for sight and hearing may occur. At several points the two systems have very direct connection; as both traverse the midbrain they are in close proximity, and both certainly send fibres to the same motor nuclei of the brain stem. For example, the auditory nuclei, particularly the superior olive and the inferior colliculus, connect with nuclei of the trigeminal, facial, and abducens nerves and thus cover the head, neck, and shoulders; while the main optic reflex nucleus, the superior colliculus, connects through the tectobulbar and tectospinal systems with the red nucleus (a visual righting-reflex center), the eye muscles which turn the head, and by way of the medial longitudinal fasciculus, to motor nuclei throughout the body. It is also thought that a still higher auditory nucleus, the medial geniculate, connects with the brain stem to mediate high auditory reflexes.

But not only do many visual and auditory paths end on the same motoneurones or intercalaries: there are fibres from the lateral lemniscus, an auditory path in the brain stem, direct to the superior or visual colliculus. There may be fibres between the auditory and visual colliculi; and there is on the cortex a tract of fibres, the inferior longitudinal fasciculus, between temporal and occipital gyri.

I dwell on these facts at length because it is a widespread belief that the rather scattered accounts of intersensory facilitation are to be explained on the basis of attention, and do not have a more direct neurological bases. But as we have seen, the idea of summation is a basic one in the theory of neural integration; in the auditory and visual systems we have seen plenty of opportunity for such summation to occur.

Experimental.

We now turn to the experimental evidence. For simple reaction-time, where we would presumably be dealing with facilitation on the motoneurones, MacDougall found reaction-time to sound was shorter in colored light than in darkness. Todd found little effect of adding light to sound. This is probably because the reaction-time to sound is already nearly at a minimum, being reduced only 9σ by adding electric shock, and only another 4σ by further adding a light. Dunlap and Wells showed that such results depend in part on whether the subject attends to the sound or the light. Jenkins showed that reaction-time to a light was facilitated in a continuous noise. In this sort of experiment, where the facilitating stimulus is not momentary but continuous, the recurrent "attention" explanation loses most of its effect. Here we must think in some such terms as in later writings of Lorente do Nó (1935), where he speaks of a "background of excitation" necessary for facilitation.

We may next look at facilitation as expressed, not in speed, but in magnitude of movement. The contribution of sound and light to the eyelid reflex has been depicted by Hilgard. When light precedes sound by 25-50σ, the sound reflex is augmented by an amount greater than that to light alone. If light precedes sound by 75σ and above, the second reflex is inhibited. Facilitation is best when the two reaction tendencies are simultaneous, although it will occur when the light reflex appears anywhere in the contracting phase of the response to sound.

We may next look at sensory acuity. Urbantschitsch in 1888 stated that sound sharpened sensitivity of the eye to light. Tanner and Anderson also made this statement. Newhall found sensitivity to momentary visual stimuli was improved by providing clicks along with the lights. Here, attention is certainly doing the lion's share in facilitation. Freund cites authors somewhat to this effect back to 1669. Hoffman took gross tests of auditory acuity under a 1000w lamp. 50% of his subjects showed facilitation. Zietz reported that tones caused visual after-images to flicker, and changed brightness and contour of figures. Reciprocally, lights influenced tones: in a lighted room, tones were judged higher in pitch. Schiller noted that auditory beats emphasized flicker, and Schiller and Wolff that low tones could be varied in 'brightness' by changing light intensity.

Child and Wendt found that a momentary light flash would most improve an auditory threshold if it preceded the sound by half-a-second. Kuroki found a similar optimum. The contribution of attention here is not determined, but is undoubtedly great.

Kravkov (1930, 1934) reasons that auditory stimulation will raise the level of excitation of the brain, and so affect the process of "irradiation" of white surfaces in the visual cortex. Thus, auditory stimulation will increase irradiation and make a white patch between two blacks even more visible, but a black patch between two white ones less visible. He found, indeed, that a 2100$\sim$ tone did increase acuity of black on white and lower that of white on black, as predicted. Hartmann (1933, 1934) agrees with Kravkov on an overflow of energy from one sense to another, but states that acuity for both white-on-black and black-on-white is improved. His theory of reciprocal flow of energy is purely cortical. Serrat and Karwoski, on the other hand, find no change in visual sensitivity under continuous tonal stimulation. At present, the subject is still very much open.

Several attempts have been made by the conditioning technique to cause a tone to elicit a sensation of color in a subject. Kelly and Cason both report in the negative. Bogoslovsky found that a metronome would increase the eye's visible response to electric current, and Howells found that tones and colors could be associated through long training. Thus, the score pro and con is at present 2 and 2.

It may not be irrelevant to note here that a sound and a light, if simply presented together several times without reinforcement, will each evoke a response later, if in the meantime only one of them is reinforced (Brogden 1939, 1942, 1947; Karn). This sensory pre-conditioning almost certainly works by way of some small intermediary response made originally to both sound and light even without reinforcement.

Conclusions on intersensory facilitation.

I have mentioned most of the experiments of scientific value on auditory-visual facilitation. They can be summarized as follows: if two sensory paths converge on a motor center, the reflex due to either may be altered in speed and in magnitude. The speed will not much exceed that of the faster path; the magnitude will depend upon the arrival times at the point of convergence. Simultaneity is best; other temporal patterns may inhibit rather than facilitate. The weight of evidence is toward one organ enhancing the sensation from another organ, though there are negative findings. Finally, the creation, so far, of synaesthesia in the laboratory is debatable, but the best opinion is that the theoretical possibility exists and is probably not debatable.

Comparison of sensitivity.

We may next turn to a comparison of how efficiently the eye and the ear collect the facts of the physical world and transduce them into sensation.

First, we may note that neither organ reaches zero sensation with stimulus intensity of zero. The eye does not report black with no external stimulus, but ideoretinal gray. Similarly, the ear manufactures its own slight sound level. With respect to external stimuli, it is remarkable that in both cases sensitivity is almost at theoretical limits. For the eye, in the scotopic periphery, this is close to the magnitude of the ultimate radiation quantum (Hecht); for the ear, this is close to the noise produced by collision of air molecules in Brownian movement (Sivian and White). Apparently, any increase in sensitivity would be biologically useless.

In terms of physical energy transduced, it is customary to regard the eye as far superior to the ear. Radiant energy, one would think, is on a different level altogether from air waves which may be so strong as even to be sensed by the skin. But it must be remembered that the eardrum has to move in and out only to the distance of 1/10th the diameter of a hydrogen molecule before a sound is heard, and the amplitude of vibration of the basilar membrane only 1/10th of this 1/10th. The fact is that in terms of energy at threshold the two organs are very roughly similar; for vision, $2.2-5.7 \times 10^{-10}$ ergs are needed and for audition, about 1×10^{-9} erg/cm^2. These values are about the weight of a mosquito's wing, dead or alive.

Comparison of range of intensities.

Proceeding from these weak intensities to the maximum which the organs can

tolerate, we find correspondingly huge ranges. The eye responds from 1 ten-thousandth to 1 billion candles per square meter, the ear from 1 ten-thousandth to a hundred thousand or more dynes per square cm, depending on the tolerance built up.

It is significant that through the rather small ranges of light and sound to which we are usually subjected, these organs both respond to intensity approximately according to Weber's law--though, in the region of maximal sensitivity, the Weber fraction is considerably smaller for vision. Thus, the eye utilizes 572 discriminable intensity steps, the ear less than 300.

No one has studied the systematic variation of the Weber fraction for auditory intensity with anything like the mathematical scrutiny to which Hecht subjected this function for the two photosensitive systems.

<u>Comparison of energy integration.</u>

We may next ask how the eye and ear compare in the integration of energy. For this comparison, we seek to know how intensity and duration are related to threshold. For both senses, duration approaches zero with infinite intensity. In other words, no stimulus can be made so short that it will not be perceived, if only it is instantaneous and intense enough (MacDougall, 1904-05; Garner & Miller). In hearing, as with vision, there is an intensity which never reaches threshold, no matter what the duration. For pure tones in a noise, this intensity is at a S/N ratio of about -20 db.

The manner in which both senses integrate energy changes from very short to longer durations. For the ear, intensity x time is constant up to 200σ; above that duration the simple reciprocity law is not quite accurate; the change in sensitivity is slight. It is interesting to note that Crozier's theory, developed for vision, predicts these auditory data as well as any other theory. He assumes that the sensitivity of individual neurones is normally distributed, and that this distribution will be pictured by the curve of sensitivity (i.e., reciprocal of intensity) plotted against log time.

For noises rather than for pure tones, intensity x time is constant for durations even longer than 200σ. Miller found the effect up to 1500σ. In this connection, note that noise stimulates a wider area than a pure tone, and recall that in vision also, the Bunsen-Roscoe law holds over a wider range for larger retinal areas (Pieron, 1920c; Graham and Margaria).

The facts of integration seem to be similar for vision. Pieron (1920a, 1920b), for example, found $IT=C$ from 1 - 70σ. For longer durations, $IT=C+D^t$, where the constant D is an adaptation effect not operating at short durations.

<u>Comparison of growth of sensation.</u>

The next problem to which we turn is that of how the two senses report a continuous stimulus. What, first of all, is the time elapsing before maximum sensation is reached? When a light is turned on instantaneously, its brilliance rises rapidly, then more slowly, and reaches a steady state without any diminution. MacDougall (1904-05) states this rise is complete in a maximum of 200σ, but this will be less with increasing intensity. I am not aware that modern vision researchers have explored this phenomenon carefully. It is something of a puzzle, for example, (and I consider it later in this paper) that although studies on single optic fibres (Hartline & Graham) show adaptation

to a continuous stimulus, nevertheless, sensation level remains constant. This cannot be explained on the basis of avalanche conduction in the retina, since a similar decline, though more long-lasting, is found in the optic nerve discharge (Adrian and Mathews). A difference situation exists for audition. The same equilibration is found for single fibres (Galambos & Davis) and for the whole nerve (Derbyshire & Davis), consisting of both a rate-adaptation and an amplitude adaptation. But, in contra-distinction to vision, the auditory sensation does decrease.

The whole question of the rise, continuance, and decay of sensation has been studied more carefully in audition than in vision.

Von Békésy studied the growth of sensation immediately after stimulation. For 800$\sim$ at about 75 db sensation level, the maximum loudness was reached in 180σ. For a much louder tone, this was reduced to 120σ; for very soft tones, the maximum was 500σ. These values are comparable to those of MacDougall for vision. Slightly longer values--over 200σ--are found by Munson for the 70 db intensity level. He has derived a formula for instantaneous loudness of a tone in terms of (1) its loudness at maximum and (2) a complicated time function. This time function is derived essentially from the adaptation rate of single nerve fibres. Munson reasons that the central nervous system contains an integrating mechanism for counting nerve impulses as they are increased due to faster pulsing rates and to additional fibres, and that some such mechanism is necessary if the brain is to use all the information supplied by the peripheral nerve.

Recently Miller has extended these observations, using noise rather than pure tones. The expected effect of intensity is found, but times are much shorter than for tones. Noise of 20 db sensation level requires about 150σ to reach maximum, one of 30 db requires 10σ less, but one of 90 db requires only about 60σ. This is, no doubt, related in some way to the fact that increased intensity of stimulation shortens the latent period of any nervous activity. It may be, of course, that the longer times for vision are due to the 100σ retinal-nerve delay interval.

In this connection we recall that Granit (1933) found the classic "four-spot" experiment holds for the retina as well as for the nerve, and that therefore this retinal-nerve interval must be synaptic, not photochemical, in origin. Now we reason that while the hair-cell response on the basilar membrane may well be chemical, such chemical action should take at least no longer than in the eye; and while lateral fibres occur on the basilar membrane, nevertheless, the peripheral ear has nothing like the 3 possibilities of lateral interaction, namely, the horizontal, amacrine, and ganglion cells, of the retina. Consequently, we might expect summative effects to be greater in the retina than in the cochlea, but conduction time also greater. And this is apparently the case.

Miller has proposed that the delay of a strong auditory sensation in reaching a maximum is occasioned simply by slower conduction of some fibres. A loud noise, which probably almost immediately fires all the fibres it is going to fire, nevertheless, takes 60σ to reach maximum sensation. He reasons that there must be some delay in transmission higher in the system, therefore.

These ideas on the growth of sensation may all be synthesized by stating that the delay of maximum sensation is caused by differences in thresholds (and therefore latent periods) in individual fibres (shown clearly by Galambos and Davis), by differences in transmission times (suggested by Miller), and by the

presence of a cortical integrating mechanism as proposed by Munson which counts and utilizes all data as it is delivered to the central nervous system.

So much for the growth of sensation. We see, however, that once a maximum is reached in loudness, a decline very quickly occurs. Von Békésy, for example, stimulated one ear with a tone of variable duration up to 150 seconds. Immediately after it ceased, the other ear received a tone of 1/5 second. This short tone was adjusted to equal the loudness of the long tone. If the first tone lasted for 10 seconds, the short tone had to be only half as intense as at first, to sound equally loud. The decline of loudness proceeded as the logarithm of time, independent of frequency.

Comparison of critical flicker frequency.

The concept of c.f.f. has been so fruitful in vision that attempts have been made to apply equivalent principles to the ear. When a tone is turned on or off, however, things happen to the physical stimulus which in the case of light can be ignored. A pure sine wave, when turned on or off abruptly, produces sidebands by scattering energy into adjacent frequency regions. Thus, no short tone can ever be "pure"; the shorter it is, the more important relatively are the unwanted frequencies. This is why very short tones sound like clicks. One cannot, therefore, by analogy with vision, simply interrupt a pure tone, varying duration and repetition rate of bursts of tone, and study the point at which a steady rather than a rough tone is heard. The analysis would be too complicated. When one interrupts a tone in this way, varying the total energy by changing either the duration of single bursts or the number of bursts per second, Garner found that the ear can hear weaker sounds if single bursts are longer, but increasing the number of bursts per second has little effect. There is a discrepancy in the way the total energy per unit time is obtained. The interpretation of this effect in neurological terms is, of course, nonsense, and means that the discrepancy is due to a radical difference in the physical stimulus when it is lengthened and when its repetition rate is speeded up.

This effect, peculiar to audition is not present, however, if noise is used rather than tones. In the case of noise, interrupting it does not change its qualitative nature, since interruption only produces frequencies which are already present; and so the direct analogue of the flicker experiment is possible. An interrupted noise seems to "flutter" (Miller and Taylor) up to a repetition rate at which it cannot be distinguished from a steady noise. This critical flutter frequency is much higher for the ear than for the eye. At only medium sensation levels, e.g. 50 db, with the sound occupying 3/4 of the on-off cycle, the c.f.f. for one subject was 300, for another subject 750 per second. For a noise-silence fraction of 1/2, the c.f.f. was around 2000/sec.

These critical flutter frequencies may be compared with their analogs in vision, as follows: for the maximum condition, namely, white light at the fovea, the c.f.f. is never much more than 50 per second. (Hecht and Verrijp). Through the middle intensity range, the eye obeys the Ferry-Porter law in that c.f.f. varies with log intensity; for the ear, the data are fragmentary but at least the c.f.f. is more nearly related to the log than to any other function of intensity. Many other questions remain to be answered, such as whether the noise-silence fraction of 1/2 is as effective in audition as it is in vision. All we can say at the moment is that the ear appears much more sensitive than the eye in handling temporally dispersed stimuli.

Comparison of bilateral interaction.

It is now fruitful, thanks to recent research by Licklider and by Hirsch in audition, to re-examine the effects of binocular and of binaural summation--or more properly, bilateral interaction. For vision, the controversy on binocular summation dates from Fechner, engaged in by Sherrington, Dawson, DeSilva and Bartley, Graham, Fry and Bartley, and Lythgoe and Phillips among numerous others, and more recently by Bartlett and Gagné. The best opinion seems to be that binocular summation, as traditionally conceived, does not occur if certain precautions are taken. Bartlett and Gagné, especially, are convincing in their discussion of possible artifacts in the work of those men supporting binocular summation.

In spite of their evidence, however, they state that it is difficult to discount the possibility under some conditions of binocular interaction; in fact, for audition the concept of binaural interaction is now well documented, as follows: for the usual audiometer technique, a 2-ear threshold is 1-2 db better than for the better ear; if the ears are equated in sensitivity, the binaural-monaural difference is about 3.6 db (Shaw, Newman, and Hirsh). However, if the phases of the 2 tones leading into the 2 ears are controlled, a surprising phenomenon results (Hirsh). If an intense noise is led to both ears, a pure tone of low frequency may then be heard better in one ear than in two ears. This is a paradox, but it is true only if the tones led to both ears are in phase (that is, both eardrums move in and out together). Hirsh calls this "interaural inhibition". If, on the other hand, the two tones are 180° out of phase (that is, both drums move the right together, then to the left), Licklider showed that binaural summation occurs, and two ears are better than one. These relations are rather complicated. For example, if the signals are in phase, the masking noise should be out of phase; and if the signals are out of phase, the masking noise should be in phase. These audibility data are strongly related to the location of the two sounds in phenomenal space. For example, if one introduces speech and noise into one ear, both sounds are localized on that side; but now if one introduces noise into the other ear in phase with the noise in the speech ear, the noise seems to "spread out" in a line through the ears, and the speech becomes more audible--though notice that even more noise has been added. In this experiment we have certain proof that the explanation for some of these phenomena must be central rather than peripheral. In other words, in audition true bilateral interaction does occur, now summation, now inhibition, as phase relations dictate.

Comparison of single nerve fibre activity.

It is now profitable to discuss sight and hearing from the standpoint of the individual nerve fibres, thanks to the initial work of Hartline and Graham, and of Granit, in vision, and of Galambos and Davis in audition. In fundamental respects these nerves obey all the laws of single nerve fibres (Adrian). Their impulse rates vary with intensity and duration for short durations. After an initial rate, equilibration sets in. Increasing the intensity shortens the latent period. And there are other similarities.

There are, of course, differences due to the structure of the two systems. For example, the auditory fibres show, in some respects, more resemblance to those of Limulus (a preparation in which a sensory cell is directly attached to a nerve fibre) than to the vertebrate eye with its retinal layers. This is true even though Lorente de Nó described certain "spinal external" fibres ending on hair cells distributed through quite a distance along the basilar membrane.

In the frog retina, a single fibre may be excited by a retinal receptive field 1 sq. millimeter in extent (Hartline, 1938). Such an area involves hundreds of receptor elements. It is true that this area is not homogeneously excitable. Nevertheless, a spot of light of sufficient intensity can discharge impulses from a single fibre in any part of this area. This situation has its direct analogue in the auditory field where a single nerve fibre has a response area to which alone it will fire. This response area, for a very intense tone, may be as much as 3 1/2 octaves wide (distributed asymmetrically about a certain fundamental frequency). At threshold, this-and only this-fundamental frequency will stimulate that fibre.

The initiation of nerve impulses in an auditory fibre depends directly upon the phase of the stimulating cycle: nevertheless, the rate of discharge of an individual fibre depends upon the stimulating intensity, no frequency. This is also true for vision; different response to wavelength is explained on the basis of the visibility curve of the receptor, not on the basis of special neural response to wavelength.

In visual fibres, the level of activity will depend, to a great extent, upon previous exposure to light. This is much less true in the case of the response to sound. Certainly, in vision, the photochemical initiation of the neural process is responsible for this difference.

For the auditory nerve, nothing can be found like the three types of fibres, namely, the steady-state fibres, the off-on fibres, and the off-only fibres demonstrated by Hartline (1938) in the optic nerve. Undoubtedly, this is related to the fact that the auditory nerve fibres are fourth-order (counting receptors as first-order). Neither is there any such diversity in the auditory system comparable to the 7 different processes which Granit (1941) found could produce nerve currents in the retina. The histology of the cochlea is simple compared with Polyak's description of the retina. If the basis for color vision is some combination of 7 types of receptor process and three kinds of nervous connection to every cone, as the work of Hartridge, of Granit (1943), and of Polyak suggests, there is certainly nothing comparable for wave-length discrimination in hearing. Finally, there is no evidence that tone-deaf ears are deficient in threshold sensitivity, as Hecht found was true for partially color blinds.

Comparison of "peripheral" and "central" explanations of acuity.

A persistent question is how the organism reports the environment with a detail much finer than the construction of the receptor organ would suggest. In audition, pitch discrimination may be good enough to suggest single inner hair cells; while in vision, vernier acuity may be about 2", or 1/30 of a single cone.

The one-to-one projection of the retina on the cortex does not mean that the grain of the cortical mosaic is exactly the same as the retinal grain. On the contrary, the unit paths are conceived as expanding cylinders, with a cellular ratio from retina to cortex of 1:1000, or an area ratio of 1:10,000. Marshall and Talbot calculate the cortical mosaic to be 30 x finer than the retinal mosaic--and this 1:30 ratio would be 20 x better if one considers (in the direction of depth) only one more layer of cells. Thus the cortical mosaic is quite fine enough to overcome diffraction as well as branching and consequent neural diffusion at lower levels. The superb acuity of vision has an explanation, then: first, in terms of normal fixation flutter producing maximum

rate-of-change of light across a single receptor, each receptor being stimulated discontinuously--a necessity in a fatiguable system; next, the nervous system, by building gradients and peaks at every edge and line of the image, acts as a lens to refocus the retinal image onto the cortex straight and sharp. Most of this activity is supra-retinal. (Marshall & Talbot).

If we may suppose with Mettler the functional organization of the auditory nervous system to be similar to the visual, then something of the same sort of explanation as the foregoing must apply for pitch discrimination. No explanation in terms of number of discriminable pitches being of the same order as the number of inner hair cells, can account for the diffuseness introduced by horizontal neural action and by increased receptor stimulation with supra-threshold intensity. The explanation in audition is simpler, since nothing like fixation flutter occurs, and phenomenal fatigue does occur. However, some neural "lens" must sharpen the relatively vague basilar pattern into the clarity of our auditory world.

It is possible that inhibition is very important here. In vision, Hartline (1940a,b) has directly observed inhibition of the "off-only" fibres of the frog. In explaining acuity, Marshall and Talbot assume a border inhibition, the mechanism of which is negative induction caused by slower waves of subthreshold excitation maintaining a depression. Other types of visual inhibition have been mentioned (Wilska and Hartline; Renshaw).

In audition, the existence of peripheral inhibition, presumably from fibres traversing the basilar membrane, was directly observed by Galambos and inferred from some experimental results by Lowy. I, myself, found evidence for inhibition in some studies on pitch discrimination in a masking noise. It seems certain that the nervous system overcomes its diffuse nature--or even uses that aspect--by setting up inhibitory processes which sharpen discrimination by emphasizing border effects of all sorts.

<u>Consideration of the quantum theory of threshold.</u>

We may now consider the quantum theory of threshold fluctuation. Hecht has shown that at threshold the fluctuation of the response of the organism to the stimulus depends more upon the variability of the stimulus than upon the variability of the organism. He has calculated that 5 to 7 quanta of light acting on the rods are sufficient to cause sensation. He suggests that wavelength discrimination may have a similar quantal explanation. The theory of the quantum in vision and light has every attraction; it is tempting to expand the concept to other vibratory phenomenon. This has been done by Stevens, Morgan and Volkmann for audition. By a curious inversion, however, these authors do not refer to the <u>stimulus</u> by the term quantum, as is common in vision, but to functional units of the nervous system. Thus, their theory (with the correctness of which we are not now concerned) is allied rather to the concepts of summation of local excitatory processes and of subliminal fringe. Thus, it is entirely incorrect to compare the quantum theory in vision with the so-called quantum theory in audition, as is often done. The two are not directly, but semantically, related.

The subject is by no means exhausted. For example, the relation between intensity discrimination and masking shows striking parallels. I cannot mention recent auditory theories of localization of objects in space in terms of cortical dimensions. Nor can I discuss the auditory analogue of Hartridge's microstimulation experiments. These relations will all be illuminating when someone works them out.

Bibliography

ADRIAN, E.D. The mechanism of nervous action. Phila.: U. Penn. Press, 1932.

ADRIAN, E.D. and LUCAS, K. On the summation of propagated disturbances in nerve and muscle. J. Physiol., 1912, 44, 68-124.

ADRIAN, E.D. and MATHEWS, R. The action of light on the eye. I. Discharge of impulses in the optic nerve and its relation to the electric changes in the retina. J. Physiol., 1927, 63, 378-414.

AREY, L.B. and CROZIER, W.J. The sensory responses of chiton. J. Exp. Zool., 1919, 29, 157-260.

BARTLETT, N.R. and GAGNÉ, R.M. On binocular summation at threshold. J. Exp. Psychol., 1939, 25, 91-99.

BASCH, S.V. Veber die Summation von Reizen durch das Herz. Sitzungsber d. K. der Wissensch. Wien, 1879, 79, 3.

BOGOSLOWSKI, A.I. An attempt at creating sensory conditioned reflexes in humans. J. Exp. Psychol., 1937, 21, 403-422.

BROGDEN, W.J. Sensory pre-conditioning. J. Exp. Psychol., 1939, 25, 323-332.

BRODGEN, W.J. Tests of sensory pre-conditioning with human subjects. J. Exp. Psychol., 1942, 31, 505-517.

BRODGEN, W.J. Sensory pre-conditioning of human subjects. J. Exp. Psychol., 1947, 37, 527-539.

CASON, H. Sensory conditioning. J. Exp. Psychol., 1936, 19, 572-591.

CHILD, I. and WENDT, G.R. The temporal course of the influence of visual stimulation upon the auditory threshold. J. Exp. Psychol., 1938, 23, 109-127.

CROZIER, W.J. The theory of the visual threshold. I. Time and intensity. Proc. Nat. Acad. Sci., 1940, 26, 54-60.

DAWSON, S. Binocular and uniocular discrimination of brightness. Brit. J. Psychol., 1913, 6, 78-108.

DAWSON, S. The experimental study of binocular colour mixture. Brit. J. Psychol., 1917, 8, 510-551.

DERBYSHIRE, A.J. and DAVIS, H. The action potentials of the auditory nerve. Am. J. Physiol., 1935, 113, 476-504.

DeSILVA, H.R. and BARTLEY, S.H. Summation and subtraction of brightness in binocular perception. Brit. J. Psychol., 1930, 20, 241-250.

DUNLAP, K. and WELLS, G.R. Some experiments with reactions to visual and auditory stimuli. Psy. Rev., 1910, 17, 319-335.

ECCLES, J.C. The discharge of impulses from ganglion cells. J. Physiol., 1937, 91, 1-22.

ECCLES, J.C. and GRANIT, R. Crossed extensor reflexes and their interaction. J. Physiol., 1929, 67, 97-118.

EXNER, S. Zur Kenntniss von der Wechselwirkung der Erregungenim Zentralnervensystem. Pflüger's Archiv. 1882, 28, 487-507.

FERE, C. Sensation et Mouvement, Paris, 1887.

FORBES, A. The modification of the crossed extension reflex by light etherization and its bearing on the dual nature of spinal reflex innervation. Amer. J. Physiol., 1921, 56, 273-312.

FREUND, L. Uber den Einfluss des Lichtes auf Funktionen des Gehör--und des Geruchsorgan. Strahlentherapie, 1929, 34, 110-116.

FRY. G.A. and BARTLEY, S.H. The brilliance of an object seen binocularly. Am. J. Ophthal., 1933, 16, 687-693.

GALAMBOS, R. and DAVIS, H. The response of single auditory-nerve fibers to acoustic stimulation. J. Neurophysiol., 1943, 6, 39-58.

GARNER, W.R. Auditory thresholds of short tones as a function of repetition rates. J. Acous. Soc. Amer., 1947, 19, 600-608.

GARNER, W.R. and MILLER, G.A. The masked threshold of pure tones as a function of duration. J. Exp. Psych., 1947, 37, 293-303.

GOTCH, F. and BURCH, G.J. The electrical response of nerve to two stimuli. J. Physiol., 1899, 24, 410-426.

GRAHAM, C.H. An investigation of binocular summation. I. The fovea. J. Gen. Psy., 1930, 3, 494-510.

GRAHAM, C.H. An investigation of binocular summation. II. The periphery. J. Gen. Psy., 1931, 5, 311-238.

GRAHAM, C.H. and MARGARIA. Am. J. Physiol., 1935, 113, 299-305.

GRANIT, R. The retinal mechanism of color perception. J. Opt. Soc. Amer., 1941, 31, 570-580.

GRANIT, R. A physiological theory of colour perception. Nature, London, 1943, 151, 11-14.

GRANIT, R. The components of the retinal action potentials in mammals and their relation to the discharge of the optic nerve. J. Physiol., 1933, 77, 207-239.

HARTLINE, H.K. Amer. J. Physiol., 1938, 121, 400.

HARTLINE, H.K. The receptive fields of optic nerve fibres. Amer. J.Physiol., 1940, 130, 690-699.

HARTLINE, H.K. The nerve messages in the fibres of the visual pathway. J. Opt. Soc. Amer., 1940, 30, 239-247.

HARTLINE, H.K. and GRAHAM, C.H. Nerve impulses from single receptors in the eye. J. Cell. & Comp. Physiol., 1932, 1, 277-295.

HARTMANN, G.E. II. Changes in visual acuity through simultaneous stimulation of other sense organs. J. Exp. Psy., 1933, 16, 393-407.

HARTMANN, G.E. The facilitative effect of strong general illumination upon the discrimination of pitch and intensity differences. J. Exper. Psych., 1934, 17, 813-22.

HARTRIDGE, H. The visual perception of fine detail. Philos. Trans, Roy. Soc. London, B. 1947, 232, 519-671.

HECHT, S. Energy relations in vision. Biol. Symposia, 1943, Vol. VII, 1-22.

HECHT, S. and VERRIJP, C.D. Intermittent stimulation by light: III. The relation before an intensity and critical fusion frequency for different retinal locations. J. Gen. Physiol., 1933-34, 17, 251-268.

HELMHOLTZ, H.V. Berichte der Berl. Akad., 1854, p. 330.

HILGARD, E.R. Reinforcement and inhibition of eyelid reflexes. J. Gen. Psych., 1933, 8, 85-113.

HIRSH, I.J. Binaural summation and interaural inhibition as a function of the level of masking noise. Amer. J. Psychol., (In press).

HOFMANN. Licht and Hören. Med. Klin., 1929, 1-5.

HOWELLS, T.H. Experimental development of color-tone synesthesia. Psychol. Bull., 1937, 34, 714.

JENKINS, T.N. Facilitation and inhibition. Arch. Psy. 1926, 14, No. 86, pp.56.

JENNINGS, H.S. Modifications in behavior. II. Factors determining direction and character of movement in the earthworm. J. Exp. Zool., 1906, 3, 435-455.

KARN, H.W. Sensory pre-conditioning and incidental learning in human subject. J. Exp. Psychol., 1947, 37, 540-544.

KATZ, B. Experimental evidence for a nonconducted response of nerve to sub-threshold stimulation. Proc. Roy. Soc., 1937, 124B, 244-276.

KELLEY, E.L. An experimental attempt to produce artificial chromaesthesia by the technique of the conditioned response. J. Exp. Psychol., 1934, 17, 315-341.

KRAVKOV, S.V. Ueber die Abhängigkeit der Sehschärfe vom Schallreiz. Arch. f. Ophthalm., 1930, 124, 334-338.

KRAVKOV, S.V. Changes of visual acuity in one eye under the influence of the illumination of the other or of acoustic stimuli. J. Exp. Psy., 1934, 17, 805-812.

KUROKI, S. The influence of a light stimulus upon hearing. Jap. J. Psychol., 1937, 12, 253-269, (English summary).

LICKLIDER, J.C.R. The influence of interaural phase relations upon the masking of speech by white noise. J. Acous. Soc. Amer., 1948, 20, 150-159.

LOCKE, F.S. Notiz über den Einfluss physiologischer Kochsalzlosung auf die elektrische Errigbarkeit von Muskel und Nerv. Centralbl. f. Physiol., 1894, 8, 166-167.

LORENTE de NÓ, R. Researches on Labyrinth reflexes. Tr. Am. Otol. Soc., 1932, 22, 287-303.

LORENTE de NÓ, R. Vestibulo-ocular reflex arc. Arch. Neurol. and Psych., 1933, 30, 245-291.

LORENTE de NO, R. The summation of impulses transmitted to the motor neurones through different synapes. Amer. J. Physiol., 1935, 113, 524-528.

LYTHGOE, R.J. and PHILLIPS, L.R. Binocular summation during dark adaptation. J. Physiol., 1937, 91, 427-436.

MacDOUGALL, R. On the influence of varying intensities and qualities of visual stimulation upon the rapidity of reactions to auditory stimuli. Am. J. Physiol., 1903, 9, 116-121.

MacDOUGALL, R. The variation of the intensity of visual sensation with the duration of the stimulus. Brit. J. Psychol., 1904-5, 1, 151-189.

MARSHALL, W.H. and TALBOT, S.A. Recent evidence for neural mechanisms in vision leading to a general theory of sensory acuity. Biol. Symposia, 1942, 7, 117-164.

MILLER, G.A. The perception of short bursts of noise. J. Acous. Soc. Amer., 1948, 20, 160-170.

MILLER, G.A. and TAYLOR, W.G. The perception of repeated bursts of noise. J. Acous. Soc. Amer., 1948, 20, 171-182.

MUNSON, W.A. The growth of auditory sensation. J. Acous. Soc. Amer., 1947, 19, 584-591.

NAGEL, W.A. Experimentelle Sinnesphysiologische Untersuchungen an Coelenteraten. Pflüg. Arch. ges. Physiol., 1894, 57, 494-552.

NEWHALL, S.M. Effects of attention on the intensity of cutaneous pressure and on visual brightness. Arch. Psychol., 1923, 9, No. 61, pp. 75.

PIÉRON, H. De la variation d l'énergie liminaire en function de la durée d'excitation pour la vision foveale. C. r. Acad. des Sci., 1920, 170, 525-528.

PIÉRON, H. De la variation de l'énergie liminaire en fonction de la durée excitation pour la vision périphérique. C. r. Acad. des Sci., 1920, 170, 1203-1206.

PIÉRON, H. De la variation de l'énergie liminaire en fonction de la surface rétinienne excitée pour la vision périphérique (cônes et bâtonnets). C. r. soc. de biol., 1920, 83, 758-756.

PIKE, F.H. and COOMBS. H.C. Functional organization of the nervous mechanism of respiration. Sci., 1922, 56, 691-693.

PIOTROWSKI, G. On the muscle-nerve physiology of the crayfish especially with regard to inhibition. J. Physiol., 1893, 14, 163-197.

RENSHAW, B. Activity in the simplest spinal reflex pathways. J. Neurophysiol., 1940, 3, 373-387.

RICHET, C. Recherches expérimentales et cliniques sur la sensibilité. Compte rendus du lab. de M. Marey, 1877, 3, 97-105.

RICHET, C. Contribution à la physiologie des centres nerveux et des muscles de l'écrevisse. I-III. Arch. Physiol. norm. path., 1879, 6, 262-294.

RICHET, C. Contribution à la physiologie des centres nerveux et des muscles de l'écrevisse. IV-VII. Arch. Physiol. norm. path., 1879, 6, 522-576.

SCHILLER, P. v. Das optische Verschmelzen in Seiner Abhängigkeit von heteromodaler Reizung. Z. F. Psychol., 1932, 125, 249-288.

SCHILLER, P. v. and WOLFF, W. Gegenseitige Beeinflussung der optischen und der akustischen Helligkeit. Z. F. Psychol., 1933, 129, 135-148.

SERRAT, W.D. and KERWOSKI, T. An investigation of the effect of auditory stimulation on visual sensitivity. J. Exper. Psych., 1936, 19, 604-611.

SETCHENOW, J. Études physiologiques sur les centres modérateurs des mouvements réflexes dans le cerveau de la grenouille. Ann. d. Sci. Natur., 1863, 19, 109-134.

SHAW, W.A., NEWMAN, E.B., and HIRSH, I.J. The difference between monaural and binaural thresholds. J. Exp. Psychol., 1947, 37, 229-242.

SHERRINGTON, C.S. On binocular flicker and the correlation of activity of 'corresponding' retinal points. Brit. J. Psychol., 1904, 1, 26-60.

SIVIAN, L.J. and WHITE, S.D. On minimum audible sound fields. J. Acous. Soc. Amer., 1933, 4, 288-321.

STEVENS, S.S., MORGAN, C.T. and VOLKMANN, J. Theory of the neural quantum in the discrimination of loudness and pitch. Am. J. Psychol., 1941, 54, 315-335.

TANNER, A. and ANDERSON, K. Simultaneous sense stimulations. Psychol. Rev., 1896, 3, 378-383.

TODD, J.W. Reaction to multiple stimuli. Arch. Psychol., 1912, 3, No. 25, pp. 65.

URBANTSCHITSCH, V. Ueber den Einfluss einer Sinneserregung auf die übrigen Sinnesempfindungen. Arch. f.d. ges Physiol., 1888, 42, 154-182.

VON BEKESY, G. Zur theorie des hörens. Physik. Zeitschr., 1929, 30, 115-125.

YERKES, R.M. Bahnung und Hemming der Reactionen auf tactile Reize durch akustische Reize beim Frosche. Pflüg. Arch., 1905, 107, 207-237.

YERKES, R.M. The sense of hearing in frogs. J. Comp. Neur. and Psych., 1905, 15, No. 4, p. 279-303.

ZIETZ, K. Gegenseitige Beeinflussung von Farbund Tonerlebnissen: Studien uber experimentell erzeugte synasthesie. Z.F. Psychol., 1931, 121, 257-356.

REPORT OF THE

SUBCOMMITTEE ON ILLUMINATION

ARMED FORCES-NRC VISION COMMITTEE

May 28, 1948

The Subcommittee on Illumination was appointed during April to cope with two specific requests for assistance presented to the Vision Committee through the Office of Naval Research. The subcommittee and Dr. H. A. Imus, representing the Office of Naval Research, visited the projects at the Arma Corporation in Brooklyn and at the Bell Telephone Laboratory in Whippany (from which the requests for assistance had originated.)

The subcommittee analyzed the problems and made specific recommendations for the projects. Details are omitted from this report because both projects are classified.

The subcommittee has been asked to continue its existence for the purposes (1) of being called into activity as need arises to deal with specific requests for help from the services, (2) of maintaining liaison with the Illuminating Engineering Society, and (3) of preparing reports of a general nature which might be helpful to persons concerned with the design of visual presentations.

The subcommittee intends to deal with the design of visual presentations and to let illuminating engineers and others work out the lighting design, interior decorating, etc., to achieve the recommended form of visual presentation.

It will be obvious that projects appropriate for this subcommittee will be undertaken by other subcommittees. For example, one subcommittee has concerned itself with the distribution of brightness in stimulus patterns for testing visual acuity. It should be emphasized that this is a subcommittee and not a supercommittee, and is a "catch-all" for problems with which no one else wants to work.

One member of this subcommittee, Dr. Blackwell, has prepared an extensive summary of data for use by the subcommittee. Eventually, this and other summaries may be made generally available under the sponsorship of the subcommittee.

Glenn A. Fry, Chairman

C. L. Crouch
H. R. Blackwell
S. B. Williams
D. K. Farnsworth

INTRODUCTORY EXPERIMENTAL EXAMINATION OF
THE EFFECTS OF ILLUMINATION ON ACUITY UNDER VARIOUS OPTOMETRIC
CONDITIONS

by

Dr. Z. John Schoen

This paper is in the nature of a critique of a project which we have, so to speak, "inherited" from the late Dr. Franklyn D. Burger. Burger, an ophthalmologist, repeatedly emphasized that his study was of a practical nature, namely to establish a screening program for the Naval services. His technique, suitable as consulting room routine, was open to serious question when applied to research. Controls were lacking or inadequate. Optimum conditions for visibility relating to such factors as relative brightness of surround, uniformity of illumination, contrast, pupillary constriction, and fixation were not established. The type of visual target used and the method of scoring precluded proper statistical treatment in the analysis of data. We know little of minor details and not enough of his principal procedure.

However, credit must be given to Dr. Burger for conceiving the study in its original form; hence, a review of his labors in this particular field is in order. The relation between intensity of illumination and visual acuity has been the subject of previous studies, but the role played by refractive error in the deterioration of acuity which results from lowering the light level had not been investigated before Burger's inception of this problem late in 1941. His interest in the problem was aroused during a survey of civilian airline pilots. Since a great number of landing accidents had been occurring in the hours just before dawn and just after dusk, he measured visual acuity at a level which approximated the brightness obtained during these periods. These tests revealed marked differences in acuity among men who possessed roughly equivalent acuities under normal testing conditions, i.e., at a light level of approximately 10 to 15 ft-L. That these differences were not caused by individual differences in dark adaptation was proved by measuring that characteristic with the Hecht-Shlaer Adaptometer. However, by comparing the acuity of any man at normal illumination with his acuity at low light levels, Burger believed he could make a fairly accurate guess as to the man's refractive error. His interest thus being centered in the relation between visual acuity under low illumination and refractive error, he proposed to examine a number of men under low illumination, and to repeat the examination in steps of increasing brightness until a standard light level had been reached.

Illumination was furnished by two Kleig ellipsoidal projectors, and the light level was varied through the use of neutral filters mounted on the periphery of a wheel at the front of each projector. Two photographic screens were placed in front of the observer to form a "v" shaped angle, but were separated at the apex to permit him to view the target. Light levels were measured in foot candles with two photovolt cells, one held at the target and the other at the eye.

Burger wished to simulate what he believed to be out-of-door conditions and insisted that the illumination at the eye should be equal to that on the target. Such light conditions decrease visual acuity and make it difficult to say at what adaptation level the tests were made. Furthermore, the target

field was not uniformly illuminated, the letters on the right side being more easily read than those on the left. In addition, the brightness mediating pupillary constriction was considerably higher than that of the test field which, viewed foveally, primarily determines retinal adaptation.

Snellen letters were used as a measure of visual acuity. There are many objections to the use of this type of target for experimental purposes, chief among them being the following: (1) the letters are not of equal legibility, (2) they involve the perception of meaningful material and measure something more complex than just retinal resolution, (3) the decrement between steps is too large and (4) the number of items varies from line to line.

No provision was made for proper fixation at scotopic levels. Presumably, scanning could have been used, but this was left entirely up to the subject, and untrained observers such as those used probably did not know how to scan.

The specific limitations of Dr. Burger's technique will become more apparent when I discuss the modifications we found necessary to impose; therefore, I now propose to give a brief highlighted summary of his results in his own words as gathered and organized from several of his principal reports. But first, a word of caution: Dr. Burger, unfortunately, has left no written analysis of his extensive graphical material, nor any detailed interpretation of data; hence, we are uncertain of the basis of his conclusions. I quote:

"Four hundred and ninety-eight men were examined. The procedure used consisted of a test of visual acuity at 24 light levels between 0.0027 foot candles and 2.0 foot candles after preliminary dark adaptation. The light on the test chart and that on the man's eye was held at the same level. Following this test, the men were refracted for the highest plus power giving maximum visual acuity when combined with any minus cylinder required by the man's refractive error. The results of these tests were analyzed and graphed by type and amount of refractive error.

"In general, this survey has shown that, in the light range tested, the best visual acuity is shown by those with a spherical error of three quarters of a diopter of hyperopia. Between three quarters of a diopter and one and three quarters diopters of hyperopia the visual acuity at these low light levels remains about the same; with more than two diopters of hyperopia there is a slight decrease in visual acuity. However, the number of subjects in the higher values is too small for significance.

"With less than three quarters of a diopter of hyperopia, the average visual acuity in low illumination decreased gradually until emmetropia is reached; then there is an abrupt drop as one quarter diopter of myopia is reached. At extremely low light levels this rapid rate of decrease is maintained to values of myopia as high as one diopter, while at intermediate levels of low illumination a plateau is reached at one half diopter of myopia, which extends to one diopter; then there is a second abrupt drop in average visual acuity.

"The visual acuity under low levels of illumination was found to vary with the amount of hyperopia present (considering myopia as a negative value of hyperopia). The presence of astigmatism in amounts up to one diopter was found to have but a slight effect on the visual acuity. It was found that if one recorded the refractive error in terms of plus sphere and minus cylinder, the visual acuity for these types of refractive error varied as would a straight spherical error which was the algebraic sum of the actual sphere and cylinder.

"With less than three quarters of a diopter of hyperopia some individuals, while by refraction hyperopes, under low levels of illumination show a visual acuity characteristic of myopes and for practical purposes must be considered as such under actual operating conditions.

"The experimental work done has been of a practical nature, merely accepting the eye as a working organ. There is, however, evidence that the lowered visual acuity of low hyperopes was due to the development of spherical aberration so that at low light levels the subject is no longer a hyperope, but is myope. This, of course, was to be expected from known facts: the pupil dilated as the light level was dropped and as the pupil dilated a more peripheral zone of the lens was exposed. This zone possessed rather high spherical aberrations which would have the effect of making the man more myopic. It would seem that this is the major factor involved.

"If we take these men and place minus lenses in front of their eyes, we can restore their visual acuity at low levels of illumination even though we are suffering a light loss of about 8% through the use of lenses. However, we can take a farsighted individual, and by making him artificially nearsighted, we can duplicate the visual acuity of the men who start out with no refractive error at all or with a small amount of nearsightedness. Also, by using an artificial pupil we are able to improve the visual acuity of these men although the light levels on the chart remain the same. Nothing has been found in the investigation so far which is not capable of explanation by this hypothesis, and no other hypothesis has been suggested which can adequately explain the experimental findings.

"Since this investigation has been of a practical nature rather than theoretical, we have not attempted a correlation of the pupil's size with the visual acuity. This would have to be done by photographing the pupils with infra-red camera, and such equipment has not been available. It does not seem to be of importance from the practical standpoint since we are not going to vary the size of the pupil in Naval Personnel; this factor, therefore, has been completely ignored.

"A few men were run through the test at levels up to 1000 foot candles, but not enough of these tests have been done to indicate any more than a steady gain in visual acuity to a level of six foot candles, then a more rapid rise to 20 ft. candles, thereafter a slow rise in all except myopes, and for all classes very little increase above 100 foot candles.

"The results indicate that a level of illumination and a level of visual acuity can be selected which will allow mass screening with a significant error in all probability of less than five percent.

"In any experiment which is designed to investigate a certain point, almost inevitably during the investigation certain byways develop which are, in themselves, well worth following and perhaps even more important than the main subject itself. In the course of this investigation there are several points of interest which have developed:

1. The central scotoma, which has long been noted as being present during night, extends further up into twilight than has been generally accepted. This scotoma is not absolute but is a formed scotoma and should be the subject of further investigation from the standpoint of perception and recognition.

2. Distance perception is definitely changed during low illumination and by correcting lenses. The extent and importance of this change would seem to be of interest. It is known that nervous tension has a considerable effect on visual acuity, and this factor should be further investigated.

3. Fatigue, also, has a similar effect, as does prolonged reading; these should be further investigated."

In plotting his data, Burger secured a family of visual acuity curves for varying amounts of myopia, hyperopia, and astigmatism. Acuity for all conditions rose as the light level was raised from 3/1000 to 1000 F.C. * His contention that emmetropes and hyperopes become relatively myopic as the light level is lowered, can best be supported by comparing the acuities of these two groups at high and at low illumination levels. Since the emmetrope, in contrast to the hyperope, has no hyperopic reserve upon which to draw as night myopia is introduced, his acuity should deteriorate to a greater extent with the drop in light level than that of the hyperope. This seems to be the case. At 100 F. C. the average acuity of hyperopes up to 1.5 diopters is 20/13; that of the emmetropes 20/14. At 20 F.C. these average acuities drop respectively to 20/14 and 20/19; however, at .01 F.C. the hyperopic acuity falls to 20/70, whereas the emmetropic acuity drops relatively much further to 20/140. Approximately equivalent in acuity at 100 F.C., the hyperope was able to see a target one half the size of that recognized by the emmetrope at .01 F.C. These results indicate a trend toward night myopia, but cannot be regarded as conclusive because very low light levels were not used, the largest target size was only 20/200, and the technique was inadequate.

In analyzing Burger's interpretation of his results, one begins to suspect that he (Burger) attributed decrease in visual acuity at reduced light levels primarily to the formation of a relative myopia as a direct result of pupillary dilation. To him, light level within the range used is a principal factor in visual acuity only as it affects pupillary size, and is apparently of secondary importance as an acuity determinant through its effect on retinal sensitivity. Moreover, although Burger speaks of very low light levels, the lowest illumination level (.0027 F.C.)**he used was actually in the region of the photopic threshold; therefore, his study cannot be considered as one involving low scoptopic vision. An interesting notation in his graphs is the point at which the central scotoma disappears. This is at an illumination level of .02 F.C., which he believes to be higher than is generally supposed.

Evaluation of Dr. Burger's investigation emphasizes the reasons why it has been necessary to re-examine its objectives, and to reformulate it in a way which will transform the study from a series of tests into an experiment. I will now point out our progress in that direction.

It is our purpose to measure the visual acuity under optimum conditions of visibility through a wide intensity of illumination range, the limits of which will be dictated by the size of our test objects and certain practicable con-

- - - - - - - - - - - - - -

* Both illumination and visual acuity were plotted on a logarithmic scale.

** It is not possible for us to transpose Burger's figures into brightness units. Moreover, his surround and target fields were not equal in brightness; hence, we cannot arrive at the levels of brightness which he used, even if we guess at the absorption and reflectance characteristics of his fields.

siderations, but which are expected to be in the neighborhood of 3.5 log uuL on the lower, and 1000 ft-L on the upper, end. The effect of refractive error, kind and degree, upon the relationship between acuity and light level will be investigated. The question of night myopia is to be especially considered in order to evolve a formula for placing men whose refractive errors will permit good night vision in billets where such vision is of paramount importance, while screening out the ineligibles. Acuity determinations at normal light levels will not ordinarily indicate what the acuity will be under conditions of lowered illumination. It is, therefore, important to know for practicable reasons at what light level nocturnal myopia becomes manifest, its degree, and to what extent it increases as illumination is decreased. Of theoretical interest is the nature of its cause. If we know these facts we will be in a better position to deal with this type of error and to make suitable recommendations regarding it for personnel selection.

Heterophoria measurements will be made at normal and low light levels for the purpose of studying the relation between accommodation and convergence * at low levels. This will provide an index of accommodation at low levels. A stigmatoscope, adapted for this study, will likewise be used to observe this factor.

Some acuity measurements will be made with optometric corrections in place to equalize acuities within a group, making it possible to compare them with emmetropic acuities. All measurements will be monocular.

We do not intend to make a "population" study for some time. At present we are repeating observations with normal or corrected acuities in order to learn what is happening at low light levels, and under the conditions we have set up. Following this, we expect to deal with spherical ametropias and simple astigmatism. At this stage we are more interested in securing a greater amount of data with fewer, but trained observers, than it will be possible to obtain later with large groups.

The first major modification of Burger's technique is in the change to geometrical non-letter visual targets of the checkerboard type. They are especially suitable for research of this nature and have none of the disadvantages of the Snellen type used by Burger. Furthermore, they provide the finest measure of retinal resolution evolved to date, as determined by the AGO studies and are considered reliable. I will not describe them extensively, since Dr. Rudolph has demonstrated their use in his report on target validation. We are using two kinds, one for scotopic and the other for photopic vision. The former is a hand-painted type, 18 1/2 inches square, mounted with the three gray comparison squares on a four foot square target holder, each of the four units occupying a corner, with the red fixation light in the center of the holder since indirect viewing is used. The target is at a distance of ten feet from the observer, since we discovered that scotopic acuity increased in using a region of the retina more remote from the fovea than could be used at the 20 ft. distance. In order to use this latter distance the target holder would have had to be doubled in size, making it extremely difficult to rotate and otherwise impracticable. Hence, our scotopic measurements will be made

- - - - - - - - - - - -

* It is doubtful whether a "fixed" convergence, as suggested by Wald, will be evidenced under low illumination unless the visual axes are "fixed" in parallel fashion with distance fixation. Otherwise, diplopia would result. An individual will sacrifice clear vision, as in over-accommodation, but he will not sacrifice single vision.

preponderantly at 10 feet. At this distance the following dimensions obtain:

1. each of the squares, from corner to corner, subtends an angle of 15.5° involving a diamondshaped retinal area at 4.5 to 20 degrees from the fovea;

2. the corner of each square unit is 4.5° from the fixation point, allowing a central rod scotoma of 9° to be absorbed;

3. the target holder circumscribed an area of 40° around the fixation point; and

4. our scotopic targets range in size from 9.5 to 107.6 minutes visual angle.

The photopic targets, to be used primarily at 20 feet with direct viewing, differ from the others in several respects. The over-all size of the checkerboard square, composed in each case of 5 alternate black and white checkers, varies with the size of the visual angle subtended by them. Consequently, they are considerably smaller than the scotopic target units, which are kept constant in size in order to use the same portion of the retina regardless of the size of the individual checkers. Another difference is that in the photopic targets the gray of the comparison fields is created by fusion of small black dots. Finally, they are photographically made, hence, required extensive validation before they could be used. They range in size from .35 to 9.5 minutes of visual angle.

Resolution of a checkerboard target requires recognition of the checkerboard square as differentiated from the three gray units of equal size and brightness. This involves correctly stating the position (up, down, right, or left) in which it appears after rotation of the target holder, by hand in the case of the large scotopic target, by remote control motor in that of the photopic ones. Twenty-four exposures* are given for each target size, the positions of the checkerboard unit being varied in accordance with a predetermined random order. Each position is used the same number of times in a series.

Illumination of the target field for scotopic testing is supplied by a Kleig ellipsoidal projector set at 15° to the normal, at which position it was possible to secure uniform brightness of target and the surround (which extended to 180°) although there was a gradual dropping off of brightness beyond the central 60° sector which primarily determines retinal adaptation. In this position, also, the contrast was greater and specular reflection less than at the 45° angle usually used.

For photopic testing at 20 feet an additional projector is used to illuminate the white walls of a semi-cylindrical enclosure, inside of which the observer sits while viewing the target through an aperture.

This cyclorana serves as the surround, which is held at a brightness equal to that of the target field. Supplementary sources of illumination will be used at higher brightness levels.

The brightness of our test field for the scotopic observations now being conducted is 1 ft-L., and is reduced through the use of neutral density filters mounted at the front of the projector. Light level measurements are

- - - - - - - - - - - -

* At present, there is no time limit for each exposure.

made with a Macbeth illuminometer. It will be noted that, in contrast to Burger, we are relating visual acuity to brightness, and not to the intensity of illumination in foot candles. This permits us to state at what brightness levels the experiment is performed.

Since scotopic testing on an experimental level is time consuming, an attempt has been made to "streamline" our procedure as much as possible. The observer is dark adapted with standard Navy goggles for half an hour, is then seated at a table adjustable for heighth to which a specially constructed head and chin rest is attached, and one eye is occluded. The experimenter rotates the target holder to the proper position as advised through a battle phone by the recorder sitting in the next room. He then exposes the target, opening a shutter mounted in front of the observer at his station. This is done by pulling a string attached to the shutter, which springs back into place when it is released. The observer makes his judgment, calls out his response into a microphone, and it is then recorded in the next room. Clamps hold the checkerboard unit in place on the target holder. This arrangement makes it possible to change the target in a minimum of time without changing the target holder. The sides of the holder are marked with fluorescent paint, and the reverse side of the targets are numbered in a similar way to obviate visual difficulties while working at extremely low light levels. The recorder and experimenter are in constant communication with each other over the battle phone system in order that such items as target size, time in the dark, and the observer's comments are immediately recorded.

To date, approximately ten observers have been tested at two light levels, viz., $\frac{3}{100{,}000}$ and $\frac{2}{100{,}000}$ Ft-L. The average acuity at the first level was 38 min. and at the second 45 min. Repeated observations showed a very small day to day variation. Although these results are only preliminary in scope, certain trends manifest themselves. For example, the blind spot seems to have some effect. The least correct responses resulted when the target was in the position upon which the normal blind spot encroached, while the best in this respect (number of correct responses) was the "up" position. The ratio of correct responses for the 3 positions as compared to the blind spot position was approximately 1.25: 1. Observers, likewise, reported that under critical visibility conditions the target was most difficult to identify when it was in the blind spot position.

The mean variation in correct responses, as target size was varied, was about the same as Dr. Rudolph found at the photopic level in his target validation series. This means that the decrement in target size may be the same for both levels. Target sizes in steps of 1/8 log unit of visual angle will not give a sharp cutoff, therefore, this is the amount of decrement we have adopted.

Another interesting finding is that a "fading" * occurs at intervals for certain observers, especially when target differentiation becomes difficult. It lasts for a period of perhaps four or five responses and disappears as suddenly as it comes. Observers state that when it occurs it is as if all the illumination had been cut off. Whether it is of cyclical nature or due to fatigue cannot be ascertained at this stage of our study. Fatigue may be a big factor in our study; therefore, we limit observations to a period of one hour. One observer stated, "If I can't see the pattern immediately on

- - - - - - - - - - - -

* Not to be confused with after images appearing as "white clouds," etc.

exposure I am unable to make a judgment. It becomes impossible!" In general, observers claim that when they are forced to make a difficult judgment it becomes almost impossible to do so. For this reason we do not force judgments.

As yet, we do not have enough data to treat statistically; in fact, we are still conducting "path finding" operations. Acuity * and brightness, however, will be evaluated on a logarithmic scale when more data is available. Lythgoe** claims that the relationship between these two variables is dependent upon the "end point", or threshold, adopted. By plotting correct responses against size of target at each brightness level, we expect to secure sigmoid curves from which it will be possible to compute the point of greatest mean variation in terms of correct responses as target size is varied. It may be feasible to use this percentage of correct responses as our threshold, although we do not at this stage feel ourselves limited to any one type of treatment. Our present data may be regarded only as indicating certain trends which may be considerably modified before the experiment is completed.

I have left to the last one of our most interesting findings. It concerns the manifestation of night myopia in the one case which we have studied in that respect. The development of a relative myopia, as light level is greatly lowered, has been found by many experimenters. In almost all instances, however, it was claimed to exist simply because concave lenses or divergent effectivities of optical systems made vision under low illumination clearer. Thus, a negative setting of binoculars, for example, was regarded as proof of myopia. Such a conclusion is not entirely justified. To begin with, minus lenses will increase visual acuity, especially in young people, by creating an artificial hyperopia, even in the daytime. Minus lenses, in such cases, are a measure of the artificial farsightedness produced by them, and not of myopia. Second, the fact that a greater negative setting is made at night than in the daytime is likewise not a valid proof, since daytime acuity, being already high, will not be greatly improved in this fashion, and a balance between visual comfort and relative acuity increase will probably be struck at a lower negative setting than at night where the artificial hyperopia will be more effective. In any case, the fact that concave lenses correct myopia cannot be taken to mean that they measure the amount of myopia present. In short, one must show that a myopia exists without the use of negative lens effectivities.

A means of proving that the image of a fixated object lies in front of the retina, as in myopia, is skiascopy, in which a retinescope is used. Unfortunately, skiascopy involves the use of a strong beam of light which is directed into the eye to observe parallax, then to neutralize it with the proper lenses. This would not do at low scotopic levels for obvious reasons. However, the temptation was strong and I did not want to reject this simple method of directly proving the existence of nocturnal myopia. It finally occurred to me that possibly a red beam could be used; I am glad to say it was a complete success. Red light skiascopy, for our purposes, possesses the following advantages over that with white light:

1. It is monochromatic, eliminating any myopia caused by chromatic

- - - - - - - - - - - - - -

* The formula for treating acuity in this manner is Log 2. $\frac{2^{10}}{A}$, where A = visual angle in minutes.

** Lythgoe, R. J. "The measurement of Visual Acuity," Reports of the Committee upon the Physiology of Vision, Medical Research Council, London, 1932.

abberation.

2. Pure red light has almost no effect on the pupillomotor fibres, so it does not eliminate myopia resulting from pupillary dilation at low light levels.

3. Because of the great difference in scotopic and photopic transmission (0.3 and 7% respectively) of the filter used, it does not interfere with dark adaptation of the eye under examination, nor, as proved by tests, with the scotopic vision of the eye regarding the target. At the same time, since the photopic transmission is some 20 times higher than the scotopic, the examiner can easily study the movements of the retinescopic reflex in the subject's eye.

Accordingly, a red filter with a "cut off" at 619 mμ was attached to the retinescope; repeated tests produced the following results:

With white beam in optometric alley, 10-15 Ft-L: + .50 D.

With red " " " " " " : + .62 D.

Under scotopic conditions at $\frac{1}{1000}$ ft-L on target. Red beam: + .12 D.

" " " " $\frac{3}{100,000}$ ft-L " " " " : – .37 D.

Monocular and binocular fixation gave the same results. When accommodation of the non-examined target-regarding eye was inhibited with convex lenses, at the scotopic levels, the result was also the same showing that no accommodation of a consensual nature was influencing the eye under examination. It will be seen that with the red beam 1/8 D. more plus was elicited than with the white.* After dark adaptation at the 1/1000 F.C. level, the hyperopia was decreased by 1/2 D. from .62 D. over that obtaining at 10-15 ft-L. At the lowest level used (3/100,000 ft-L.), the hyperopia was replaced by 3/8 D. of myopia.

In short, 1/2 D. of relative myopia was developed at the intermediate level and 1 D. at the lowest level.** Since the experimental error in using the retinescope is about 1/16 D., these results definitely indicate the onset of relative myopia in this one case, increasing in degree as the brightness level is dropped.

It is doubtful whether accommodation played any part in the development of the night myopia as measured with the retinescope, since accommodation was inhibited with convex lenses. The size of the pupil increased approximately 2 to 3 mm. in going from the high to the low level. Therefore, one may strongly suspect spherical aberration as the causative agent, although the negative spherical aberration of the flattened peripheral section of the cornea is a compensating factor and must be considered in reaching this conclusion.

- - - - - - - - - - - - - -

* This was to be expected, inasmuch as red is less refrangible than white light. Unexpected was the small difference, although great individual differences exist in this respect. Only the results obtained with the red beam at the three light levels were compared.

** That is, for red light. For white light, especially in the scotopic eye, this amount would be greater.

NIGHT VISION TESTING

Dr. Scobee asked the Committee to discuss the problem of night vision testing. This discussion was requested in order to take action upon a letter received from General Hargreaves, Deputy Air Surgeon, copy of which is presented below:

> "It appears that additional procurement of night vision testing apparatus will be necessary in the very near future.
>
> "The opinion of the Army-Navy-NRC Vision Committee is requested as to whether, within the framework of present standards and test procedures, specifications can be devised rapidly for an instrument which might be acceptable to all the Armed Services.
>
> "It is realized that research and development will require changes in testing techniques and apparatus in the future. However, with the existing emphasis on induction and cross-servicing procedures, a common test within our present framework is believed highly desirable.
>
> "If it is the opinion of the committee that a common night vision testing apparatus is presently feasible, further recommendations concerning specifications are requested."

Discussion

Dr. Scobee raised the general question of the interest of the various military services in night vision.

Major Rostenberg outlined the Ground Force interest during the last war in night vision testing and training, and reported some of the results obtained at Fort Blanding.

Dr. Verplanck stated that he believed there to be very little purpose in a night vision testing program. He felt that a training program is far more important, since he believed that the improvement in night vision to be expected from training is large with respect to individual differences in original ability. He felt that a night vision training program was absolutely necessary for any duties in which night vision is essential. If the training program is obligatory, he felt that the best screening could occur at the time of the training program.

Colonel Byrnes stated his belief that Dr. Verplanck's suggestion would be unnecessarily costly since many duties in the Air Forces, for example, necessitate night vision. If men were not screened for night vision before the general training program was begun, expensively trained men might be dropped late in training as a result of the screening in the night vision training program.

There was considerable discussion concerning the degree of night vision necessary for pilots, with Dr. Grether tending to minimize the degree, and Colonel Byrnes emphasizing the importance of adequate night vision.

Dr. Fry asked whether sufficient experiments had been run to determine the

difference in night sensitivity dependent upon the exact method of testing.

Dr. Hulburt commented that two sets of laboratory experiments, those at Tiffany and those at NRL, reported measurements of point source thresholds at night obtained with very different techniques. The surprising fact was the close agreement of the two sets of data, apparently in spite of the differences in measuring procedure.

Dr. Blackwell reported that two separate experimental procedures had been employed at Tiffany, one utilizing indefinitely long search, and one utilizing relatively short search, each of which yielded thresholds very similar to those obtained at NRL.

Dr. Marquis asked whether Dr. Johnson would care to comment on the RCAF night vision research.

Dr. Johnson first commented on the necessity for testing night vision. He stated his belief that the elimination of the pathologically night blind was not difficult since these individuals show clinical symptoms. He believed that among those with "normal" night vision, differences between the best and worst men were relatively small. Dr. Johnson emphasized the the importance of motivation and training as opposed to basic differences between individuals.

Dr. Tousey suggested that the differences in night vision ability of various people might be related to the incidence of spherical aberration.

Dr. Chapanis suggested that there was considerable evidence of normal distributions of visual functions which would indicate a continuous distribution of ability, and would suggest the desirability of selection for night vision ability.

Dr. Verplanck emphasized the many factors leading to variability in test results on night vision examinations. He emphasized, particularly, day to day variability sensitivity.

Captain Willmon commented on the long history of night vision testing at New London. He stated that the conditions at New London were probably optimal compared to most field installations, and yet, that rather gross variability on retest was often obtained. Captain Willmon expressed his general agreement with Colonel Byrnes' concern for proper selection of personnel. He stated that perhaps the results obtained to date merely indicate that an acceptable night vision test has not been developed.

The question of night vision testing was referred to the Subcommittee on Visual testing for further discussion and for recommendations to be made to the Air Surgeons.

ABSTRACTS

199. <u>Target Indication from the PPI of the SG-1b (MOD 50) Radar.</u>
N. R. Bartlett
Systems Research Field Laboratory and Psychological Laboratory, The Johns Hopkins University.
Report 166-I-24, 1 September 1947, 58 pp. (R)

"Purpose. Such studies as this one are intended (1) to provide an appraisal of capacities of new radar equipment, as well as the best means of operating it, and (2) to develop methods for the appraisal of such equipments. Facilities, however, did not permit the appraisal of new equipments and required that the test be made of the SG-1b (mod 50) radar, which is by no means new equipment. As the most realistic compromise, therefore, the study was so designed as to fulfill these two objectives in a more general way, that is, (a) by evaluating the capacities and best operating conditions for PPI's having the same general characteristics as the PPI of the SG, and (b) by canvassing as thoroughly as possible the kinds of test measurements, and methods of analyzing data that might be applicable in the study of other similar PPI's.

"Method. Although the SG-1b radar might be used without the antenna continuously rotating and with scopes other than the PPI, in order to make the study more general and to economize in experimental times, only conditions of continuous rotation of the antenna and only the use of the PPI were studied.

"The principal measures of the effectiveness of target indication from the PPI were (a) maximum frequency of reports, (b) error of range readings, (c) error of bearing readings, and (d) the average time delay between appearance of a fully visible target pip and the completion of a report.

"Oral transmission of target information was used throughout the experiments because that has been the most usual Naval practice in target indication and because most of the conclusions do not depend on the type of transmission used, whether oral or mechanical.

"Measures of effectiveness were made with respect to several variables: (a) number of simultaneous raids, (b) antenna rotation speed, and (c) the use of reading aids, such as the bearing cursor and range trace, alone or in combination.

"Four rated Navy radarmen served as operators in the experiment. One ten-minute "battle problem" was conducted with each operator for each of 60 permutations of antenna rotation speed, number of raids, and reading methods used. An exhaustive analysis of the results was carried out, leading to many detailed conclusions, which are described in the main text of the report. The most significant conclusions, stated in general form, are given here:

<u>Conclusions and Recommendations</u>

"Antenna Rotation Speed. In general, higher rotation speeds are to be preferred to lower speeds, for they do little harm and may do some good.

1. The higher speed always increases number of reports for single

raids; and for three or more raids, it will increase them by two percent if the range trace is used, and make no difference if the trace is not used.

2. Again, the higher antenna speed decreases slightly the range error if the range trace is used and increases error somewhat if the trace is not used.

3. Raising antenna rotation speed decreases the bearing error for single raids and makes no difference in bearing error if there are three or more raids.

4. If the range trace is not used, raising antenna rotation speed decreases markedly the average delay time of reporting, and does so to a lesser extent when the range trace is used.

"From the data obtained in this study, however, it appears that an antenna rotation speed of nine or ten rpm is about maximal for the advantages of the higher speed. It is doubtful if increasing the speed beyond ten would make any difference in operator performance in regard to frequency of reports, bearing or range error, or time delay.

"<u>Frequency of Reports</u>. The limitation of the maximum frequency of reports that can be obtained is the traffic-handling capacity of the operator. All other factors are minor.

1. Frequency of reports will be increased by increasing the antenna rotation speed if there are only one or two targets; for three raids or more, there is a negligible effect.

2. Frequency of reporting reaches its maximum at two to three raids, regardless of other conditions. In general, operators can handle orally only 10 to 12 target reports per minute no matter what the conditions.

3. The maximum is a little lower when the range trace is used than when it is not.

4. Frequency of reporting is decreased slightly by using the bearing cursor without the range trace, but it is increased slightly by using the bearing cursor in addition to the range trace.

"<u>The Use of the Bearing Cursor</u>. There is little merit or disadvantage to the use of the bearing cursor. There are slight advantages offset by slight disadvantages so that the net result is to make it unimportant whether the bearing cursor is used or not.

1. When used with the range trace, the bearing cursor increases slightly the frequency of reports: when used alone, it decreases them slightly.

2. It adds slightly to bearing accuracy (.2 to .3°), and also adds a trifle to range accuracy when used in conjunction with the range trace.

3. It consistently adds a little in the average time delay of report-

ing targets, and this of course, adds some error of reading of moving targets, thus offsetting any slight advantage in the accuracy of readings.

"The Use of the Range Trace. Most of the facts are definitely in favor of the range trace and only one--the cost in time to use it--is against it definitely.

1. There is a substantial reduction in frequency of reports by using the range trace for low antenna rotation speeds (four to six rpm) when there is more than one raid. At higher antenna rotation speeds, however, the decrease in frequency of reports from using the range trace is small.

2. Without the range trace, error increases substantially--more than twofold--as the number of raids is increased; with the range trace, range error is held at its minimum irrespective of the number of raids.

3. The average delay in reporting targets is held to four or five seconds if the range trace is used but may get as high as nine seconds (with single raids) if it is not used. Time delay in reporting is always less, by one to four seconds, when the range trace is used."

200. Accuracy, Variability and Speed of Adjusting an Indicator to a Required Bearing.
E. L. Kaufmann, T. W. Reese, J. Volkmann, and S. Rogers
Psychophysical Research Unit, Department of Psychology and Education, Mt. Holyoke College, South Hadley, Massachusetts
Memorandum Report 166-I-MHC 4, 2 September 1947, 37 pp. (0)

"This report is one of a series on the judgment of bearing, or angular orientation about the center of a circular display. A knowledge of how these judgments of bearing are made, their accuracy, their variability, their speed, is basic to the understanding of one of the fundamental perceptual tasks in the CIC's.

"In the studies reported here, the subjects were shown a large circular display on which was a line of light (similar to a bearing cursor). They were told a certain bearing and their task was to adjust the line of light to what they thought was that bearing. They were allowed all the time they wished, but their time to adjust was recorded.

"All six subjects showed a surprising degree of accuracy even though the display contained no grid or other indication of bearing. This accuracy was greatest and variability least at the vertical, and the horizontal. There was also an increase in accuracy and a decrease in variability in the region of 45 degrees.

"Although normal individuals have a fairly clear notion of the location of the vertical and horizontal, they differ greatly in the way they go about making interpolations between these values. They may resort to complicated subjective bisections and trisections of the quadrant as well as subjective additions and subtractions of angular distances in order to reach a satisfactory judgment.

"Speed of adjustment is maximum in the region of the vertical and the horizontal and is increased somewhat in the region of 45 degrees.

"The presentation of a single bearing marker increases accuracy over a limited span of bearing and decreases variability over a wide span. The marker also has a pronounced effect on the speed of adjustment. This effect is not only a substantial local increase of speed in the region about the marker, but also a more general, though somewhat less pronounced, increase over the whole span of bearings.

"When subjects adjust to a bearing told them by the experimenter, they are more accurate and less variable than when they give direct verbal estimates of a bearing shown them by the experimenter. This may be explained by the fact that in these experiments the subjects were allowed more time to adjust than to estimate."

201. Signal Mark Size and Visibility of Radar Signals on a Plan Position Indicator.
N. R. Bartlett and S. B. Williams
Psychological Laboratory, The Johns Hopkins University.
Report 166-I-30, 20 September 1947, 16 pp. (0)

"Preliminary research on the detectability of signal images on a 7BP7 radar screen indicates the extreme importance of the image size. Image size was varied (1) by regarding the radar screen from different distances, (2) by changing the length of the image by widening the azimuth gate, (3) by changing the thickness of the image by switching to different presentation range scales and (4) by moving the signal from the center to the periphery of the screen. Image size variations by such methods can change the strength of the minimum visible signal by at least eight or ten decibels. The data are preliminary, for equipment limitations did not permit variations from a signal pulse length of two microseconds nor reductions in azimuth gate below 12 degrees. Further research on the problem of image size with refined equipment is now in progress."

202. Design of a Semiautomatic Night Vision Scotometer.
R. H. Lee and R. H. Draeger
Naval Medical Research Institute, National Naval Medical Center.
Project X-467, Report No. 1, 21 July 1947, 5 pp. (0)

"The scotometer described is an instrument designed to measure the size of the central scotoma of the dark adapted eye. Radial measurements of separation of test light and fixation point are made along eight equally spaced meridians. Choice of meridian and test light settings are accomplished mechanically. Test light, fixation point and indicator dials are self-luminous.

"Two subjects were tested on the scotometer. It was found that the boundaries of the central scotomata of right and left eyes were nearly identical in both subjects. It is suggested that this symmetry of retinal pattern of the two eyes is intimately associated with the mechanism of binocular summation and binocular convergence in the dark adapted eye."

203. Field Tests of Optical Instruments.
W. S. Verplanck
U. S. Naval Medical Research Laboratory, U. S. Naval Submarine Base, New London, Conn.
NavOrd Report 77-46, 15 March 1947, 331 pp. (R)

"This report described the methods employed and the results obtained in a large-scale field test of optical instruments. The purpose of these tests was not to supersede laboratory tests, but, rather, to supplement and verify their results in order to assist in the design and selection of such instruments. The findings of this report show that the laboratory testing of optical instruments must be supplemented by a large-scale field test before valid conclusions may be drawn. It has been found, as a result of these field tests, that some of the conclusions reached solely as the result of laboratory tests have been misleading. Laboratory findings, although precise and essential, can not cover all of the variable factors which affect the performance of optical instruments when used by naval personnel under operating conditions.

"The results of these tests show the need for additional field work of this type, and also show that future design and selection of optical instruments should be based on field as well as laboratory results."

204. Appraisal of an Experimental PPI Presenting Simulated Bearing, Range, and Height Information.
J. W. Gebhard and E. A. Bilodeau
Systems Research Field Laboratory and Psychological Laboratory, The Johns Hopkins University.
Report 166-I-37, 15 December 1947, 53 pp. (R)

"Two experiments were performed to appraise a conventional polar coordinate display modified to present bearing, range, and height information. The display consisted of a PPI surrounded by an annulus. Each target was represented by two blips on the same bearing: the inner blip on the PPI gave bearing and range, and the outer blip in the annulus gave bearing and height. The display was a synthetic or simulated display. Engineering modifications actually to reproduce bearing, range, and height information on this PPI were not accomplished. The purpose of this appraisal is a psychological one: to determine how accurately a man can understand and extract information from a display of this sort.

"The first experiment measured the speed and accuracy with which operators could extract bearing, range, and height information from the experimental display on three remote repeaters: (1) a VJ used normally with the bearing cursor and a movable range marker, (2) a VJ equipped with four range and four height markers without cursor, and (3) a VG with a standard eight ring overlay. The average time per target required on either the VJ with markers or the VG was less than half that required for the VJ with hand cranks (nine seconds compared with 21). The average bearing error obtained on the VG was one degree and on both VJ's about a degree and a half. The average range error on a 40-mile scale was about one-half of a mile for all indicators. The average height error on a 40-thousand foot scale was about 500 feet.

"The second experiment measured speed and accuracy of target estimation for the experimental display presented on a VH (5 in.), VD (7 in.), VJ (12 in.), and VG (24 in.) display, each equipped with four range and four height rings.

No cursors were used: bearing was estimated directly from the dials. Two range-height scale ratios on the radius were tested for each instrument: 1:1 and 2:1. The average time per target was about seven seconds except for the VG which took a second longer. The average bearing error was about one degree for the VG. It became larger with a decrease in scope size and reached three and a half degrees for the VH. The average range error on a 40-mile scale was a little over half a mile for all scopes and the average height error on a 40-thousand foot scale was about 700 feet. The 2:1 ratio was found better for estimating bearing and range, but the 1:1 ratio was best for height.

"These experiments indicate that the extraction of bearing, range, and height information from a radar indicator can be rapidly and accurately accomplished by operators using the PPI-and-annulus display. The display is satisfactory on scopes as small as five inches, but bearing, range, and height accuracies are improved on somewhat larger scopes. The presentation is readily comprehended and experienced radar men can use the modified scopes with little practice."

205. Mounting Angle of a VJ Remote Radar Indicator and Its Effect on Operator Performance.
M. Leyzorek
Systems Research Field Laboratory of The Johns Hopkins University at Jamestown, Rhode Island
Report 166-I-41, 10 February 1948, 12 pp. (R)

"A VJ remote radar indicator was mounted at seven angles (scope-face with respect to the floor: 0°, 15°, 30°, 45°, 60°, 75°, 90°,) and tests were made to discover whether there is an optimum mounting angle for operator performance. The results of the experiment show that the angle at which a VJ is mounted has no significant effect on an operator's speed and/or accuracy in target indication.

"A questionnaire was given to the group of subject-operators used in the experiment to determine which mounting angle they preferred. An analysis of these results gave the following rank order (from most to least preferred): 15°, 45°, 30°, 60°, 0°, 75°, 90°. The 0° position is the conventional mounting angle."

206. The Accuracy of Range Estimation on a PPI with Respect to the Distance of the Target from Range Rings.
J. D. Reed and N. R. Bartlett
Psychological Laboratory of The Johns Hopkins University.
Report 166-I-29, 1 November 1947, 9 pp. (R)

"Four radar operators estimated the location of targets on simulated PPI's. Their data were analyzed for accuracy of range estimation as a function of target position with respect to range rings. The grouped data show that the average accuracy of estimation changes in a complex fashion as the distance from the range ring to the target increases. In general, targets near range markers and those near the center of the inter-ring distance are judged more accurately than those at other places."

207. The Design of Numerals for Use in Counter-Type Instruments: A Review of the Literature.
J. G. Gleason
Division of Education and Applied Psychology, Purdue University.
Report 166-I-39, 20 December 1947, 24 pp. (0)

"The works reviewed in the preceding sections appear to provide adequate information concerning the characteristics of the variables studied. It is felt that the restudying of these variables using numerals as test-objects will not add greatly to the facts already available. Rather, it is felt that the preceding summary of the findings of various experimenters in the field of visibility yields considerable data that may be applied directly to the design of numerals for counter-type dials. These may be summarized as follows:

"1. The size of the critical detail in the numerals should be as large as is possible within the necessary practical limitations of overall numeral size. Within these limitations, it may be said that the larger the numeral of any given type, the greater will be its visibility; the larger it is, the more rapidly it can be identified, the larger it is, the smaller the contrast ratio required to be seen, the larger it is, the lower the brightness level required to be seen.

"2. The contrast between the numeral and its immediate background should be as great as it is possible to provide. By increasing this contrast ratio for any given numeral type, equal visibility may be maintained when the size is reduced, or the brightness level is reduced, or the time for apprehension is reduced.

"3. The brightness contrast between the dial area and the surround should preferably yield a ratio of one. If this is not possible, a reduction of surround brightness below the level of the dial area reduced legibility only slightly. A situation which yields surround brightness greater than dial areas is to be strenuously avoided as this condition greatly reduced acuity.

"4. The brightness level of the numeral and its background should be as high as is possible in the situation obtaining. Increasing the brightness level increases the speed and accuracy of apprehension and with the same type numeral a given visibility level may be maintained by increasing the brightness level while decreases in contrast are made or decreases in size are made, or while time for apprehension is decreased.

"5. For greatest visibility, other factors held equal, numerals should be either black or white on opposite background. In order of decreasing visibility, in all cases inferior to black on white or white on black, are black on yellow, yellow-green, orange, green, red, blue-green, and blue. The question 6f the relative merits of black on white and white on black has not been conclusively answered as yet.

"In brief then, to provide a counter dial of the greatest possible visibility, each of the above factors should be applied to the greatest extent compatible with other requirements of the situation.

"Two other variables exist in the use of numerals, in addition to those presented above, which have not been adequately investigated. These are style of numeral and width of stroke used in the numeral. Little experimental data is available in which these have been analyzed. Therefore, our problem in the study of the design of numerals for use in counter-type instruments is conceived

to be the study of these two factors while holding the already investigated variables constant."

208. Some Considerations of High Intensity Approach Lighting.
H. J. Cory Pearson and M. S. Gilbert
Civil Aeronautics Administration, Technical Development, Indianapolis, Ind.
Technical Development Report No. 60, March 1948, 23 pp.

"This report discusses the high intensity approach lighting problem and various types and applications of lights that are being offered as solutions to the problem.

"It shows that under conditions of restricted visibility a light having a relatively moderate candlepower value can be seen from a certain maximum distance. As this candlepower is multiplied many times, the distance from which the light can be distinguished is increased very little, until a limit is reached beyond which it is impractical to increase the candlepower. There are other factors which enter into the case. The background illumination is very important, as a light that could be seen against a bright background such as prevails in daylight would be so uncomfortably glaring as possibly to blind a pilot trying to land at night with lamps of that brightness, and, conversely, lights of proper brilliancy for normal night flying would be invisible under daylight conditions. These facts, known to engineers, determine the limits to which the designer may go in selecting his tools for solving the problem.

"It is obvious that there is need for brightness variation control to provide adequate brightness for visibility under diverse conditions, such as those prevailing during daytime fogs as compared with nighttime.

"Various approach lighting systems that have been, or are being tried out are discussed and criticized. The old systems had faults which the new systems attempt to eliminate by various means. There are different methods for accomplishing the same results.

"The only system which attempts to indicate to the pilot his position with respect to the glide path is the proposed CAA slope-line system. This is thoroughly discussed and explained.

"With the aid of knowledge gained through exhaustive experimentation, it is believed that the engineering profession soon will find the optimum solution to the approach light problem."

209. Special Problems in the Estimation of Bearing.
T. W. Reese, J. Volkmann, S. Rogers, and E. L. Kaufman
Psychophysical Research Unit, Mt. Holyoke College, South Hadley, Mass.
Memorandum Report 166-I-MHC 2, 15 January 1948, 41 pp. (0)

"This report covers three special problems concerned with the estimation of bearing, or angular orientation around the center of a circular display. Two previously published reports have presented the basic data on the accuracy and variability with which judgments of bearing are made. In the three experiments reported here, the subjects were shown a line of light projected on a large circular display and were asked to estimate the bearing of this line in degrees.

The line of light was exposed for one-half second and the subjects were allowed 4 seconds in which to make their estimates. Under these general experimental conditions the following special questions were asked:

"1. What effect has the length of the line of light whose bearing is to be judged on the accuracy and variability of the subject's judgments? (The line whose bearing is to be judged is referred to as the <u>bearing</u> <u>indicator</u>).

"2. What happens to accuracy and variability when only the outer tip of the bearing indicator is shown?

"3. What is the size of the individual differences in both accuracy and variability, and is there any relation between accuracy and variability, that is, do the most accurate subjects tend to be the least variable?

"The answers to these questions are, very briefly: First, changing the length of the bearing indicator over a wide range (from 93% of the radius of the display screen to only 6.6% of the radius) had little effect on either the accuracy or the variability of the subjects' estimates. Secondly, the outer tip seems to be the most effective portion of the bearing marker in making accurate judgments of bearing. In fact, under the conditions of this experiment, accuracy is increased and variability reduced when only this portion of the marker is shown. Thirdly, the most accurate subject was about three times more accurate than the least accurate subject. The most variable subject was about 3.5 times more variable than the least variable subject. Finally, there is a correlation between error and variability of +0.41, indicating that the most accurate subjects tend to be the least variable in their estimates; conversely, the most inaccurate tend to be the most variable."

210. <u>German Claim Regarding Effects of Helenien on Night Vision.</u>
<u>(Evaluation by Flt. Lt. Bazarnik, R. A. F. Institute of Aviation Medicine)</u>
Gt. Brit. FPRC. Vision Committee, Minutes of the 37th Meeting, 17 February 1948, FPRC 694, 10 pp. (0)

The claim of Von Studnitz that administration of helenien (prepared from marigold) would produce significant increases in rod sensitivity has been carefully reinvestigated. The administration of helenien to a group of subject with normal night vision produced no modification in either dark adaptation or other visual function including defective color vision. The data obtained from Von Studnitz suggest that his subjects had visual thresholds higher than normal. The effect obtained by him is the well-substantiated improvement in threshold of Vitamin A deficient subjects with Vitamin A therapy.

211. <u>Comparative Performance of Commercial Screening Devices and Far and Near Wall Charts Utilizing the Same Visual Test Targets.</u>
<u>E. B. Cook</u>
U.S. Naval Medical Research Laboratory, Submarine Base, New London, Conn.
27 May 1948, (0)

Frequency distributions for all test scores and tabular presentation of statistical results.

212. Effects of Benzedrine Sulphate on a Visual Vigilance Test.
N. H. Mackworth and F. H. Winson
Great Britain, MRC, Royal Naval Personnel Research Committee
R.N.P.47/401, O.E.S.129,A.P.U. 72, September 1947, 9 pp. (O)

"10 mgms. of Benzedrine Sulphate, taken by mouth one hour before the start of a two-hour visual vigilance test, prevented the appearance of the deterioratio in accuracy normally to be expected after half an hour on this type of work. There was, however, no sign at any time that the subjects were better than their normal or untired level of performance. It was rather that this level was maintained over a longer period than usual.

"The 24 subjects missed more than twice as many signals without Benzedrine (16%) as they did when they had been Benzedrinized (7%). This effect was present in most of the subjects, since 20 of the 24 subjects made fewer errors with Benzedrine.

"This was a psycho-pharmacological result and was not due to the suggestion effec of having taken some unspecified tablets. There was no real difference between the accuracy of the men without any tablets at all compared with their accuracy with inert tablets.

"No "hang-over" effect on accuracy was detectable the day after taking this moderate dose of Benzedrine.

"The men were about one-fifth of a second quicker at pressing a key in answer to the visual signals with Benzedrine (1.05 seconds) than they were without Benzedrine (1.24 seconds).

"It is not known whether these improvements in accuracy and speed of work would have been found if Benzedrine had been repeatedly used in this way.

"Stringent precautions must always be taken in the repeated use of Benzedrine to avoid habit formation.

"During actual routine look-out duties, it would therefore seem much more satisfactory to prevent loss of vigilance by keeping the length of visual watch as short as possible (the optimum length being half an hour) rather than by considering the use of Benzedrine."

213. The Design and Display of Pilots' Instruments.
H. P. Ruffel Smith
GT. Brit. RAF. FPRC, Institute of Aviation Medicine
FPRC 692, March 1948, 19 pp. (R)

"The present position regarding instruments in the cockpits of R.A.F. aircraft is reviewed with reference to recent trends and new instruments which are within the scope of possible adoption for future use in the Service. Topics dealt with are:

1. The pilot's perception of instruments and instrument development.
2. Some general principles of display.
3. Particular instruments:
   (i) "Flight" instruments.
   (ii) Approach and landing instruments.
   (iii) Engine monitoring instruments.

4. Relations between instrument design and cockpit dimensions.
5. A suggested panel giving all the information required for blind flying.
6. The assessment of improvement in instrumentation."

214. Proposed Specifications of Red and Green Navy Signal Lights.
Dean Farnsworth
U. S. Naval Medical Research Department, Submarine Base, New London, Conn.
Color Vision Report No. 16, 1 March 1948, 17 pp. (O)

"Some thirty or more separate specifications for red, green, yellow, and white signal lights were in use in the United States, Great Britain, France, and the Netherlands at the beginning of World War II. When their land, maritime, and air forces were joined, certain differences had to be adjusted and compromised. In addition, new specifications were adopted, sometimes hastily, to accommodate newly developed instruments or changing techniques.

"New studies on visibility of colored signal lights by normal persons were made during the war which furnished data which had not been available when most of the specifications were originally written. New data was furnished on the appearance of colors at small subtense. The participation of large numbers of partially red-green blind men in the Services raised the question of the possibility of adjusting color signals for better recognition by the color weak. There was an increase in Naval and amphibious operations which required the use of signals at minimum brightness secured by reduction of current supplied to the lamps.

"All of the above factors are to some extent responsible for renewed activity in the re-examination of the present diverse standards. It is expected that they can be made more satisfactory from the standpoint of manufacture and usage, and, when possible without sacrifice of efficiency, that substantial uniformity can be achieved by substituting a few codes for the many now in use."

215. Relative Efficiency of Goggles for Dark Adaptation.
Z. John Schoen and F. L. Dimmick
U. S. Naval Medical Research Department, Submarine Base, New London, Conn.
Progress Report No. 1 on BuMed Research Project NM 003 024 (X-757(Av-387-k))
"Comparison of Three Measures of Night Vision", 9 April 1948, 10 pp. (O)

"The relative efficiency of three types of goggles in securing dark adaptation was measured and the effectiveness of goggles in general evaluated in order to be able to use the most efficient device for dark adapting observers.

"Measurements were made with 19 observers under two conditions of light adaptation: Series I: 5 minutes of light at 15 mL brightness followed by 20 minutes of goggle adaptation at the same brightness, and Series II: 10 minutes of light at 300 mL brightness followed by 20 minutes of goggle adaptation at that brightness.

"Effectiveness of goggle adaptation was measured in terms of the time needed to attain two specific levels of scotopic sensitivity, namely the capacity to see an area at 4.14 log uuL luminance and one at 3.34 log uuL. These brightnesses correspond roughly to "overcast starlight" and to "very dark overcast night".

"1. Navy red goggles proved most effective in securing dark adaptation to the brightness levels used; the "browrest" red goggles stood next in efficiency and

the neutral filters were least effective, although the differences were not large.

"2. The finding that neutral goggles were consistently inferior to either of the red types under all conditions of the experiment, supports the view that the red goggles, by virtue of their lower scotopic transmission values, are superior to neutral filters for dark adaptation purposes.

"3. Adaptation with any type of goggles used here shortens the time in the dark room required for a given level of dark adaptation, the amount of time saved depending on the initial adaptation level and the level of dark adaptation it is desired to attain. The higher the intitial adaptation level the greater the relative effectiveness of red goggles.

"4. The general usefulness of goggles for the purpose of dark adaptation is dependent upon the activities required of the adapting individual. Only if he gains by being occupied while adapting, are goggles a time saver. If he must reach a certain level of dark adaptation in the shortest possible time, total darkness is more effective.

"5. For fluoroscopic and photographic dark room purposes, the small bulk and the wearing comfort of "browrest" goggles outweigh their slightly lower efficiency."

216. <u>Visibility Studies of Exterior Color Schemes for Aircraft at Present Aluminum Color.</u>
Wagner, H. G. and I. C. Blasdel
U.S. Navy, Bureau of Aeronaútics, Aero Medical Equipment Laboratory, Naval Air Experimental Station, Philadelphia
Report TED NAM AE-525044, 18 May 1948, 60 pp. (0)

"The purpose of this study was to establish a suitable color scheme for those aircraft which are at present aluminum colored, to render them more visible against background conditions when airborne.

"Preliminary studies and tests showed that background conditions against which airborne aircraft may be viewed can be given equivalents on the grey scale ranging from white to black. In selecting a suitable color scheme preference was given to the standard Navy paints of which glossy sea blue was selected as the component to be used with aluminum. Studies were then undertaken of the visibility of patterns of glossy sea blue on aluminum against backgrounds of white, grey, and black.

"The results of 22,176 observations of 37 different combinations of paint schemes on model aircraft viewed under controlled laboratory conditions by 122 different observers show that glossy sea blue applied to the trailing halves of the empennage and wing surfaces improved the visibility of an aluminum colored aircraft more than any other scheme tested."